PROCEEDINGS

OF THE

American Philosophical Society

HELD AT PHILADELPHIA

FOR

PROMOTING USEFUL KNOWLEDGE

VOLUME LIII

1914

PHILADELPHIA
THE AMERICAN PHILOSOPHICAL SOCIETY
1914

PRESS OF
THE NEW ERA PRINTING COMPANY
LANCASTER, PA.

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AMERICAN PHILOSOPHICAL SOCIETY

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VOL. LIII JANUARY–MAY, 1914 No. 213

ON "PSYCHOLOGY AS THE BEHAVIORIST VIEWS IT."

BY E. B. TITCHENER.

(*Read April 3, 1914.*)

When we speak of a science, we have in mind a logically organized body of knowledge that has resulted from certain methods of attacking the problems presented by a particular subject-matter. The methods of science are all, in the last resort, observational; the problems of science are all, in the last resort, analytical. The subject-matter of a given science may be indicated in two different ways: by a simple enumeration of objects, or by a characterization of the point of view from which the science in question regards the common subject-matter of all science, namely, human experience. Thus we may say that our psychology will deal with such things as perceptions, feelings, thoughts, or we may say that psychology, dealing "in some sort with the whole of experience," is to be distinguished as "individualistic" from other sciences which are "universalistic." It is clear that a characterization of this kind, though it necessarily transcends the limits of the science in order to show how those limits are drawn, is far more satisfactory than a mere list of objects; and psychology, these many years past, has therefore had recourse to it.[1]

[1] J. Ward, "Psychology," Encyc. Brit., XX., 1886, 38 (and later); R. Avenarius, "Bemerkungen zum Begriff des Gegenstandes der Psychologie," *Vjs. f. wiss. Phil.*, XVIII., 1894, 418; H. Ebbinghaus, "Grundzüge der Psych.," I., 1897, 8 (and later editions). On the general subject, cf. E. B. Titchener, "Psychology: Science or Technology?", in *Pop. Sci. Mo.*, LXXXIV., 1914, 39 ff.

Instead, however, of calling psychology with Ward the "science of experience regarded objectively from the individualistic standpoint," or with Avenarius the "science of experience in general, so far as experience depends upon System C," or with Külpe the "science of the facts of experience in their dependency upon experiencing individuals," or something of that sort, we are accustomed to speak of it as the "science of mind." No harm would be done, if we and our readers always remembered what "mind," as used in a scientific context, must mean. Harm begins at once when we forget that scientific meaning, and start out from the common-sense or traditional significance of the word; when we equate "mind" with "consciousness," which we take as the equivalent of "awareness," and when we set off a group of "conscious phenomena" as the peculiar subject-matter of psychology. I do not think that modern psychologists can fairly be charged with neglect of their duty to correct these errors; it seems to me, on the contrary, that our leaders are painfully careful to set their house in logical order. But habits of speech are inveterate, and common sense is extraordinarily tenacious of life: small wonder, then, that misunderstandings should arise. It is, for example, a misunderstanding that has prompted the polemical paragraphs of Watson's recent articles on what, I suppose, we must be content to call Behaviorism.[2]

This doctrine, as set forth by Watson, has two sides, positive and negative. On the positive side, psychology is required to exchange its individualistic standpoint for the universalistic; it is to be "a purely objective experimental branch of natural science" in the sense in which physics and chemistry are natural sciences.[3] It is to concern itself solely with the changes set up, by way of receiving organ and nervous system, in muscle and gland.[4] It is differentiated from its sister sciences of life partly by its special point of view, partly by the goal which it strives to attain. The changes which it

[2] J. B. Watson, "Psychology as the Behaviorist Views It," *Psych. Rev.*, XX., 1913, 158 ff. (to be referred to in the future as *A*); "Image and Affection in Behavior," *Jour. Phil. Psych. Sci. Meth.*, X., 1913, 421 ff. (to be referred to in the future as *B*).

[3] *A*, 158, 176 f.

[4] *B*, 427 f.

studies are to be approached from the point of view of adjustment to environment; its categories are stimulus and response, heredity and habit.[5] Differentiation, however, is not to be understood as separation; there is now no barrier between psychology and the other "natural" sciences; in the long run behavior will appear as a matter of physical and chemical causation,[6] while nevertheless, as behavior, it is the subject-matter of the special science of psychology, to be interpreted and arranged under the rubrics just mentioned. The erection of this special science is both justified and made possible by the practical goal of behaviorism, which is the working out of general and special methods for the control of behavior, the regulation and control of evolution as a whole.[7]

On the negative side, again, psychology is enjoined by the behaviorist to ignore, even if it does not deny, those modes of human experience with which ordinary psychology is concerned, and in particular to reject the psychological method of introspection. "Consciousness in a psychological sense" may be dispensed with;[8] consciousness, in the sense of a tool or instrument with which all men of science work, may be utilized by the new psychology without scruple and without examination.[9] Imagery, the "inner stronghold of a psychology based on introspection," is denied outright; one of Watson's "principal contentions" is "that there are no centrally initiated processes."[10] And if consciousness may be dispensed with, self-observation and the introspective reports that result from it are to be treated in even more summary fashion; they are to be "eliminated."[11] There will be no real loss; for most of the essential problems with which psychology as an introspective science now concerns itself are open to behaviorist treatment, and the residue may "in all probability be phrased in such a way that refined methods in behavior (which certainly must come) will lead to their solution."[12]

[5] *A*, 167, 177.
[6] *A*, 173, 177.
[7] *A*, 158, 162, 168, 177.
[8] *A*, 161, 163, 175, 176 f.
[9] *A*, 175, 176.
[10] *B*, 423. The statement is qualified in a footnote; I return to the point later.
[11] *B*, 428; *A*, 158, 163, 166, 170, 175.
[12] *A*, 177; *B*, 428.

Such, in outline, is "psychology as the behaviorist views it." Watson, of course, goes into some amount of detail, offering illustration and personal explanation, as well as attacking the method and problems of current psychology. But before I follow him on these various paths, I should like to record two general impressions that the reading of his articles has made upon me. The first impression is that of their unhistorical character; and the second is that of their logical irrelevance to psychology as psychology is ordinarily understood.

I call the articles unhistorical because they give no hint that any similar revolt against an established psychology had taken place earlier in psychological history. Yet one need go no farther back than Comte to find a parallel. Comte's rejection of introspection has often been referred to: let me now quote another passage in which he sums up his attack upon ideology.

"It is evident, first, that no function can be studied but with relation to the organ that fulfils it or to the phenomena of its fulfilment; and, in the second place, that the affective functions, and yet more the intellectual, exhibit in respect of their fulfilment the peculiar characteristic that they cannot be directly observed during the actual course of this fulfilment, but only in its more or less immediate and more or less permanent results. There are then only two different ways of studying scientifically such an order of functions: we must either determine, with all attainable precision, the various organic conditions on which they depend,—and this is the chief object of phrenological physiology; or we must observe the consequence for conduct of intellectual and moral acts,—and this belongs rather to natural history . . .; these two inseparable aspects of one and the same subject being, of course, always so conceived that each may throw light on the other. Thus regarded, this great study is seen to be inseparably connected on the one hand with the whole . . . of natural philosophy, and especially with the fundamental doctrines of biology; and, on the other hand, with the whole of scientific history, of the animals as well as of man, and even of humanity. But when, by the pretended method of psychology, we discard absolutely from our subject-matter the consideration both of the agent and of the act [that is, of the organ of function and of the result of its exercise], what more is there left to occupy the mind than an unintelligible logomachy, in which merely nominal entities are everywhere substituted for scientific phenomena . . .? The most difficult study of all is thus placed at once in a state of complete isolation, without any possible point of support in the simpler and more perfect sciences, over which it is proposed, on the contrary, to give it sovereign rule

On these two points, all psychologists, however extreme their differences in other regards, are found to agree."[13]

Not Watson himself could be more outspoken or more severe! But we need not go back to Comte and the thirties; we need go only to Cournot and the year 1851. After a sharp criticism of introspection, Cournot writes:

"So we see that the most useful observations on the intellectual and moral nature of man, observations gathered not by philosophers disposed to theories and systems, but by men gifted with the true spirit of observation and prepared to grasp the practical side of things,—by moralists, historians, men of affairs, legislators, instructors of youth,—have not as a rule been the fruit of a solitary contemplation and an internal study of the facts of consciousness, but far rather the result of an attentive study of the behavior (*conduite*) of men placed in various situations, subjected to passions and influences of all sorts."[14]

Here we are hardly without the circle of those "fifty-odd years" which Watson believes—how mistakenly!—have been "devoted to the study of states of consciousness."[15] It would not be difficult to cross that line;[16] but it is unnecessary. My point is that Watson's behaviorism is neither so revolutionary nor so modern as a reader unversed in history might be led to imagine; and that as psychology has weathered similar proposals in the past,—and, I hope and think, has benefited by the storm,—so also it may weather and be benefited by this latest trial of its staunchness.[17]

[13] A. Comte, "Cours de philosophie positive," III., 1838, 774 ff.; the translation of H. Martineau ("The Positive Philosophy of Auguste Comte," 1856, 383 f.) is here inadequate. The polemic against introspection will be found in "Cours," I., 1830, 34 ff.

[14] A. A. Cournot, "Essai sur les fondements de nos connaissances," etc., II., 1851, 319.

[15] *A*, 174. I have shown in my "Experimental Psychology" that the experimental period falls into fairly well-marked sub-periods.

[16] I have especially in mind Lange's chapter on "Scientific Psychology" (1866) and Maudsley's on the "Method of the Study of Mind" (1867 and later).

[17] "Should human psychologists fail to look with favor upon our overtures and refuse to modify their position," Watson writes, "the behaviorists will be driven to using human beings as subjects and to employ methods of investigation which are exactly comparable to those now employed in the animal work" (*A*, 159). The "overtures" seem to consist in the familiar "Ducky, ducky, come and be killed!" But, that apart, why should anything

The second general impression that I record is that of the logical irrelevance of Watson's programme to what is currently called psychology. For suppose that that programme were carried out to its last detail: how would introspective psychology be affected? Why, those who were interested in the method and results of introspection would simply start out where Watson had left off; the universalistic psychology being completed, it would be in order for the individualistic to be begun. A shift of standpoint over against the world of experience means the appearance of a new subject-matter, or (more strictly) of a new aspect of the common subject-matter; and any one aspect has the same claim to scientific consideration as any other; nor is there in science a Congregation of the Index to allow this and to forbid that. The behaviorist may, if he will, ignore "consciousness in a psychological sense"; he may use consciousness as a tool without making it "a special object of observation"; there is none to say him nay; but why should not some one who is not a behaviorist scrutinize what he has ignored, and try to find out empirically of what materials this particular tool is made? Logically, so far as I can see, behaviorism is irrelevant to introspective psychology. Materially, I believe that psychology will be furthered by it, since increased knowledge of the bodily mechanisms, of anything that pertains to Avenarius' System C, means greater stability of certain parts of the system of psychology. Neither logically nor materially can behaviorism "replace" psychology.

Impressions, however, must give way to closer argument: we must view Watson's articles at shorter range. And we shall, perhaps, make most progress if we begin with his pronouncements regarding the failure of experimental psychology.

Psychology, we are told, has failed signally, during the fifty-odd years of its existence, to make good its claim as a natural science. Its present condition is chaotic. The chances are that such ques-

that the "human psychologist" does or fails to do "drive" the behaviorist to do anything? I hope that Watson will find the opportunity to employ human subjects; I hope that he will find them (he will pardon the word) intelligent; I shall be honestly interested in his results.

tions as those of the extensive attribute of auditory and the intensive attribute of visual sensations, or the differences obtaining between sensation and image, will be debated two hundred years hence as inconclusively as they are debated today. Psychological method is esoteric. It has proved unable to grapple with such matters as imagination, judgment, reasoning, conception; these topics have simply become threadbare with much handling. Functional psychology is at fault no less than systematic and structural psychology. Only those "branches of psychology which have already partially withdrawn from the parent," and which are consequently less dependent upon introspection,—experimental pedagogy, the psychology of drugs, the psychology of advertising, legal psychology, the psychology of tests, and psychopathology,—are vigorous growths. The complete elimination of introspection from these disciplines will make their results still more valuable, and will keep them—as psychology itself emphatically is not—in touch with "problems which vitally concern human interest."[18]

That, I believe, is a fair statement of Watson's position; it is given largely in his own words. I have to reply, first, that fifty-odd years is not necessarily a long period in the history of an experimental science. It is not long, of course, regarded as mere duration: for it is in the sixteenth century that "the physicist abandons scholastic speculation and begins to study nature in the language of experiment,"[19] while it is only in the middle of the nineteenth that psychology becomes experimental. It might be long, in a transferred sense, if it were crowded with workers: but the number of productive students in "systematic, structural and functional" psychology does not compare with the number in physics or chemistry.[20] Has Watson, I wonder, ever counted the number of experimental papers that deal with imagination, judgment, reasoning and conception? It is notoriously difficult to trace beginnings; but we shall not

[18] *A*, 163, 176; 165; 164; 163; 173 ff.; 165; 169 f.; 170, 176.

[19] F. Cajori, "A History of Physics," 1899, 27.

[20] Mr. H. G. Bishop has kindly listed for me the experimental papers in psychology, physics and chemistry recorded in the last five volumes of Fock's *Bibliographischer Monatsbericht*. The ratio is approximately 1 : 9.5 : 44. Account is here taken of the psychological studies to be found under "Medizin," as well as of those under "Philosophie und Psychologie."

have gone far wrong if we date the first overt attempts to bring these complexes under experimental control from 1902, 1901, 1908 and 1903 respectively,—if we say, at any rate, that their experimental study belongs to the present century. And we have already worn such topics threadbare? I should rather judge that we have hardly touched their fringe. How many decades or centuries they will engage the attention of psychologists, I do not know; the important thing is that we should do thoroughly such work upon them as can be compassed in a generation. Our descendants may ask so much of us; but we owe them nothing more; and though I also hope that two hundred years hence other questions may have replaced those of visual attributes and imaginal characters, of orientation in the rat and of the homing sense of terns, I am far more deeply concerned to sift the materials of discussion than to hurry debate to a conclusion.[21]

There remain the seceding branches, experimental pedagogy and the rest. In their regard, I think, the unhistorical nature of Watson's paper renders his exposition seriously misleading; it is psychology, and not behaviorism, that has shaped their course; and it is psychology, and not behaviorism, that they still look to for guidance. Meumann's *Lectures,* for example, are offered as an introduction to experimental pedagogy and its psychological foundations; the work is penetrated with psychology; the pedagogical experiment is said to be "for the most part the psychological experiment applied to the developing and working school-child."[22] But it is largely owing to Meumann that experimental pedagogy flourishes. Rivers chose the subject of his Croonian Lectures with the desire to show that experimental psychology may be of service to medicine.[23] Stern, who

[21] It is, perhaps, beyond my province to defend functional psychology; but I should not like to have written this sentence: "It is rather interesting that no functional psychologist has carefully distinguished between 'perception' (and this is true of the other psychological terms as well) as employed by the systematist, and 'perceptual process' as used in functional psychology" (*A*, 165). What, then, of Brentano, and of the many psychologists who have been inspired by him?

[22] E. Meumann, "Vorlesungen zur Einführung in die experimentelle Pädagogik und ihre psychologischen Grundlagen," I., 1911, 27.

[23] W. H. R. Rivers, "The Influence of Alcohol and Other Drugs on Fatigue," 1908, 1, 121.

stands to the psychology of testimony in somewhat the same relation that Meumann bears to experimental pedagogy, is also through and through psychological. Binet, whose name is inseparably connected with the psychology of tests, might fairly be called an extremist in his devotion to introspection. Pick demands "eine psychologische Vertiefung der Aphasielehre," and makes constant use of laboratory material: "es ist höchste Zeit dass die Pathologie endlich von diesen Dingen Kenntnis nehme."[24] It is worth noting that Meumann, Stern and Binet—the men to whom we are chiefly indebted for experimental pedagogy, the psychology of testimony, and mental tests—would all have been brushed aside by Watson, a few years ago, as typically introspective psychologists; and it is worth noting also that they themselves look upon this later work, not as the negation of their psychological training, but as its direct extension and practical fulfilment. It is worth noting, again, that a man of Pick's authority ascribes the unprogressive state of psychopathology in large measure to an ignorance of current introspective psychology, and himself makes definite use of the "imageless thought, attitudes, and *Bewusstseinslage,* etc.," which Watson contemns.[25] I am not here depreciating behaviorism; but I think there is no justification for behaviorism's depreciation of psychology.[26]

[24] A. Pick, "Die agrammatischen Sprachstörungen: Studien zur psychologischen Grundlegung der Aphasielehre," 1913, I., 11, 58, etc.

[25] *A,* 163. The psychology of advertising, so far as it has gone, bears out my argument. Cf. D. Starch, "Principles of Advertising," 1910; W. D. Scott, "The Psychology of Advertising," 1912; W. A. Shryer, "Analytical Advertising," 1912; H. L. Hollingworth, "Advertising and Selling; Principles of Appeal and Response," 1913. The psychology of these works is not always of the severest type; but the attitude of the writers is unmistakably psychological.

[26] I have said nothing of the "esoteric" nature of introspection, because I have dealt with that charge in recent articles (*American Jour. Psych.,* XXIII., 1912, 427 ff., 485 ff.). In referring to my own work, Watson falls into the common mistake of confusing observation with theory. If he were to serve as observer in one of our studies on attention, he would have no difficulty, after a little practice, in passing the sensory judgments that we required of him. That is a matter of observation and report. Whether he would, after such participation in the actual work, accept our setting and interpretation of the results is another and a different question.

In his second article Watson discusses two topics "which may seem to many to be stumbling-blocks in the way of a free passage from structuralism to behaviorism." These topics, one sees with some surprise, are Image and Affection: with surprise, I say, because we had already been prepared to ignore consciousness and to eliminate introspection. It turns out, however, that the difficulty is methodological. For if the physiological counterpart of the image is cortical, then that mode of behavior which is to replace the introspective psychology of thought lies inaccessible within the skull. If "affection is a mental process distinct from cognition (*sic*)," then affection cannot be an "organic sensory response." So image and affection have to be dealt with; and Watson deals with them faithfully; the existence of the image is denied outright, and affection is carried willy-nilly to the periphery.

Watson offers three bits of evidence for his contention that "there are no centrally initiated processes." In the first place there are experimentalists who maintain that thought-processes may go on independently of imagery. In the second place there is no objective experimental evidence of the presence of different types of imagery. In the third place even the structuralists seek to reduce higher thought-processes to groups of obscure organic processes. I think that these arguments can be met in terms almost as brief as their statement. In the first place, the view that thought is independent of imagery hardly constitutes a presumption that there are no central processes of any kind. In the second place Fernald does not deny type, but asserts that "an individual's type can be adequately indicated only by an extended statement";[27] and that is the opinion now generally held by psychologists. But let us suppose that types cannot be indicated at all: by what logical inference may we pass from this negative finding to the denial of imagery? In the third place the reduction of thought to organic processes always implies in the background a cortical set corresponding to the *Aufgabe*. Watson, nevertheless, denies that there are centrally initiated processes, and proposes to find the behaviorist equivalent of thought in movements,

[27] M. R. Fernald, "The Diagnosis of Mental Imagery," *Psych. Monogr.*, XIV., 1, 1912, 128 ff.

chiefly, of the larynx. In the same way he finds the behaviorist parallel of affective process in tumescence and shrinkage of the organs of sex. These views are put forward as matters of hypothesis and of personal conviction, though they are also put forward with some confidence. Time and trial will prove their value.

Meanwhile, it would seem that Watson has in both cases, in the case of image as in that of affective process, overshot the logic of his position. The negative argument as regards imagery can never be proved in formal logic, to say nothing of the fact that it conflicts with a very large body of positive observation.[28] Logical confusion is shown plainly enough in the following remark: "I may have to grant a few sporadic cases of imagery to him who will not be otherwise convinced, but I insist that the images of such an one are sporadic, and as unnecessary to his well-being and *well-thinking* as a few hairs more or less on his head." If there are any images at all, then there are (on Watson's own showing) centrally initiated processes, and behaviorism is bound to take account of them; and his personal assurance that they are unnecessary to thought is offset at once by the assurance of Watt and others that thought does in fact go on in imaginal terms.[29] Science is concerned with empirical facts; and for the individual man of science to "insist" that certain facts of observation may be cancelled without loss to the science to whose subject-matter they belong is to incur, at the very least, the charge of a certain rashness of behavior.

Another logical objection seems to me to lie against Watson's procedure in this second article. All science works upon assumptions, psychology no less than the other sciences. Münsterberg, for instance, is wholly within his logical rights when he assumes that all conscious contents, without exception, may be transformed into sensations:[30] given his premises, they must be so transformed. Be-

[28] I quote a recent statement: "From an actual count of factors present in the recall of ten of our problems, we estimate that our investigation embraces approximately 200,000 images. . . . Of all our introspective data, about ninety per cent. are visual images" (E. O. Finkenbinder, *Amer. Journ. of Psych.*, XXV., 1914, 81).

[29] H. J. Watt, "Experimentelle Beiträge zu einer Theorie des Denkens," *Arch. f. d. ges. Psych.*, IV., 1905, 312; cf. my "Thought-processes," 1909, Lect. I.

[30] H. Münsterberg, "Grundzüge der Psychologie," I., 1900, 331.

haviorism would be equally within its logical rights in assuming that all central processes may be transformed into peripheral: given Watson's premises, they must be so transformed. But you cannot eat your cake and have it too. You may bring up facts in support of your choice of assumptions; and you may show the scientific results to which those assumptions lead; you may not, surely, offer these results, even hypothetically, as facts in proof of your assumptions. If we take up Münsterberg's position, we find nothing but sensations to work upon; but that is not evidence that Münsterberg's position is well-chosen. If we take up Watson's position, we find, perhaps, laryngeal movements and changes in the state of the sex-organs; but that discovery gives no logical support to the principles of his behaviorism.[31] It is, indeed, obvious that, if the larynx and the sex-organs prove refractory, the behavioristic equivalents of image and affection must just be put—hypothetically, again—somewhere else; and so on, and so forth; for it is a logical consequence of the position that somewhere on the periphery the required movements and changes are to be discovered; and the periphery is complex enough to suggest any number of localizations.[32]

[31] I do not deny that the empirical consequences of a particular theoretical attitude may serve *materially* to justify that attitude for its special day and generation; men have often worked successfully for a time though the logical foundations of their work were insecure. But the permanence of the structure depends on the solidity of the foundations, and to shirk their inspection is only to make "more haste" for the sake of "less speed."

[32] The reduction of pleasantness-unpleasantness at large to sheer sex-feeling is to me nothing else than nonsensical. But, like Watson, "I shall not attempt to develop the point further at the present time." It is, however, necessary to point out that the method of expression is not so ill bestead as Watson declares it to be. In his latest tabulation (*Arch. f. d. ges. Psych.*, XXXI., 1914, 27 ff.), E. Leschke finds 90 per cent. of substantial agreement in the investigations which he considers. The two principal sources of error are a disregard of neurasthenia and of vasomotor anomalies and—an inadequate psychological training of experimenter and observer!

I may, perhaps, be expected to say a word on Watson's criticism of my own doctrine of affection. The doctrine itself, I regret to say, he has not understood. But he has also mistaken the motives which led me to adopt it. My view that affection lacks the attribute of clearness is, he says, an assumption "arrived at largely in the interest of obtaining a structural differentiation between sensation and affection" (*B*, 426). As if a structural system would not be greatly simplified and, as system, improved by the reduc-

But the argument does not end here. I have formulated my criticism as if Watson's views were rigorously worked out, and as if his centrally initiated processes were conceived rigorously as physiological. That is, evidently, not the case; these processes are, in Watson's thought, both mental and physical; not only are brain-changes to be transformed into their equivalent peripheral changes, but the facts of psychology (as psychology is currently taken) are also to be carried, by way of behavioristic substitution, to the bodily periphery. The "required" peripheral changes are required—by the thoughts and emotions of an introspective psychology! And with that, by definition, behaviorism has nothing to do. The confusion here is plain, and the critical point need not be further labored. I must add, however, in the same connection, that I do not understand Watson's attitude to sensation. He admits that there are special cutaneous nerves "which mediate pain." He thinks that imagery is the key of the introspective stronghold: "all the outer defences might be given over to the enemy." These utterances seem to imply that sensation, if not part of the subject-matter of behaviorism, is at least neutral ground between that and introspective psychology; whereas, in the earlier article, sensation was definitely assigned to psychology.[33] Logically, I do not see how a behaviorist, in Watson's sense, can know anything of pain. I regard sensations as introspective material on precisely the same level with images; and I should challenge the behaviorist to replace or duplicate, in his universalistic terms, the various observations recorded, for example, in Stumpf's "Tonpsychologie," or in Hering's new "Lichtsinn."[34]

tion of affection to organic sensation! I only wish that I could see my way clear to it. J. R. Angell recognized the temptation in *Philos. Rev.*, XIX., 1910, 322; Watson's comment puts the cart before the horse.

[33] *A*, 164.

[34] C. Stumpf, "Tonpsychologie," I., 1883, Vorwort; E. Hering, "Zur Lehre vom Lichtsinne" [1874], 1878, 72, 106. "Der . . . Weg, welcher von den Aetherschwingungen ausgeht, hat bis jetzt, so weit es sich nicht blos um die Schicksale der Lichtstrahlen in den optischen Medien, also lediglich um eine Application der physikalischen Optik auf's Auge handelte, noch zu keinem Ergebnisse geführt"; "Ich war immer der Ansicht, dass die grossen Aufgaben, welche der Physiologie und insbesondere der Nervenphysiologie gestellt sind, am zweckmässigsten, ähnlich einer Tunnelbohrung, von zwei Seiten zugleich in Angriff genommen werden, nämlich nicht nur von der physikalisch-chemischen Seite, sondern auch von der psychischen."

All in all, this paper on Image and Affection, while it is written with a truly scientific candor, shows, I think, that the author has imperfectly grasped the logic of the situation which he has himself created.

In trying, now, to appraise Watson's proposals as a whole, we must begin by clearing them of their personal and accidental accompaniments. Watson demands a psychology "which concerns itself with human life" and whose "problems vitally concern human interest." He ascribes to such a psychology the practical goal of the control of behavior, the regulation and control of evolution in general; that is to say, he connects it with euthenics and eugenics. These expressions give his proposed psychology the stamp of a technology: for science goes its way without regard to human interests and without aiming at any practical goal; science is a transcription of the world of experience from a particular standpoint, deliberately adopted at the outset and deliberately maintained; the pursuit of a practical end is the earmark of a technology. And how does that matter in the present context? It matters very greatly. Watson is asking us, in effect, to exchange a science for a technology; and that exchange is impossible; for a technology draws not upon one but upon many sciences, and draws upon many other sources than science; and so the striking of a balance-sheet between a given science and a given technology is out of the question. I said above that behaviorism can never replace psychology because the scientific standpoints of the two disciplines are different; we now see that Watson's behaviorism can never replace psychology because the one is technological, the other scientific. This technological coloring, while it strengthens the emotional appeal of Watson's plea, is nevertheless not of the essence of behaviorism. The behaviorist's position, as we shall see, may be outlined in the plain black and white of science.

The two articles are characterized, again, by the recurring note of hurry, of impatience. Fifty-odd years gone, and we have accomplished so little: two hundred years, and shall we have accomplished much more? Surely it would be well to sweep the field clear, to forget the past, and to start the race anew! But all reformers, I

suppose, are likely to be impatient; and their impatience does not affect the value of their proposed reforms. We need not regard this hurry, either, as of the essence of behaviorism. Watson himself, in less fervid mood, might not grudge us a little time for the study of his plans,—would even recognize, I believe, that our hasty acceptance of them, without due consideration, must be more dangerous than a reasonable delay.

So we come at last to behaviorism itself; and what I take that to be I can best indicate by a parallel. In the disciplines which we call physiological psychology and psychophysiology we are interested, with slight difference of emphasis, in the two aspects of certain phenomena of the living organism; we seek to couple physiological with psychological, psychological with physiological, and so to get a complete description of the psychophysical. We may, now, in just the same way, speak of biological psychology and of psychobiology; indeed, those terms are already in use, and their general significance is plain. But here is the context to which behaviorism, if I understand it aright, must of necessity belong; it is the biological side of a biological psychology or of a psychobiology; I cannot make it more, and I do not think that its practitioners can make it less. The argument is as follows:

The behaviorist, as Watson describes him, also studies certain phenomena of the living organism. In theory, he may study these phenomena in either of two different ways. He may regard them as phenomena simply, as last facts, as things given, as phenomena to be taken at their face value and described and explained in their own right: then, he is working in what we are accustomed to call biology; he has adopted no new standpoint and needs no new name. Or again he may regard them as symptomatic; as reporting, expressing, indicating, leading up to something beyond themselves; as claiming detailed study, not only in their own right as data of biology, but also because of this further and specific character of report or expression. Here is ground for a discipline other than biology; a novel point of view has been attained. At once, however, the question arises: What, then, is it that the phenomena report or express? Of what are they symptomatic? The answer seems obvious: they are symptomatic of

behavior. And the answer seems satisfactory—until we remember that the phenomena, by hypothesis, *are* behavior, "behavior material," "behavior data," and that a phenomenon cannot both "be" and "be a symptom of" the same thing. I see no way out of this dilemma. Either the behaviorist is just biologist; and in that case he has no nearer relation to psychology than have his coworkers who are content to call themselves biologists: or the behaviorist sees expression where the biologist sees ultimate fact; and in that case he may equally well be called psychobiologist, seeing that the phenomena expressed or reported by the organic changes which he studies cannot be anything else than psychical.[35]

But if this conclusion is sound, it means two things. It means that behaviorism is correlated with a psychology, with some sort of psychology in the usual sense; and it means that behaviorism must take account of all kinds of organic changes, and not merely of those occurring at the periphery. I believe that both of these consequences must be accepted. Consider again, for example, Watson's reduction of thought to delicate movements of the larynx: those movements are movements of incipient or vestigial articulation. But words, as Watson seems to have forgotten, are also meanings; and meanings take us either to the nervous center—or to psychology; they take us, in fact, to both. Moreover, the very problem of these laryngeal movements is given to the behaviorist by psychology: how would he have lighted on the idea of transforming thought into movement unless psychology had made him acquainted with thought? I do not say that the incentive will come always or must necessarily come from the psychological side; there will be give and take; but it is none the less clear that behaviorism and psychology are, in this context, correlative; and that though an individual student may wisely and successfully confine himself to the study of behavior,—yes, and may all his life maintain a polemical attitude to psychology proper,—it is yet impossible to have a science of behaviorism independent of all psychology. It is equally impossible, of course, within the same context of psychobiology, to have an independent science of psychology; the two halves are essential to the single whole; and the psychology of

[35] Cf. with this paragraph *A*, 158 ff.

the behaviorist will, in matters of selection, emphasis, arrangement, terminology, perspective, differ from general psychology just as behaviorism itself differs from general biology.[36]

We thus conclude that to say, as was said above, "psychology would begin where a completed behaviorism left off," is really to say too little. The psychology which is correlated with behaviorism begins when behaviorism begins, and the fortunes of the two are bound up in the same bundle. Psychobiology will run the same course as psychophysiology and psychophysics. It is now, I suppose, in its first phase, when pioneer work brings in gross and tangible returns. Next will come the period of revision, of elaboration of details,—a period of discouragement, perhaps, as the former was a period of elation. And then will follow the period of slow and steady progress, varied by a certain amount of wholesome interruption. Meanwhile introspective psychology, which is now entering upon this third stage of its scientific career, will go quietly about its task, wishing the new movement all success, but declining—with the mild persistence natural to matters of fact—either to be eliminated or to be ignored.

[36] At this point we become involved in the controversy regarding the possibility of an "animal psychology." I have no wish to avoid that issue, though I must postpone its full discussion for another time. I believe that an animal psychology is definitely possible; I think that with the law of continuity as basal presupposition, and with the argument from analogy for use in the concrete case, the science may be established. Meantime I have elsewhere expressed my agreement with Watson that there can, in strictness, be no objective criterion of the psychical (*A*, 161).

PROC. AMER. PHIL. SOC., LIII. 213 B, PRINTED JUNE 18, 1914.

THE VALENCE OF NITROGEN IN AMMONIUM SALTS.

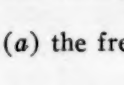
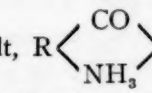
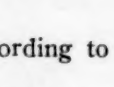
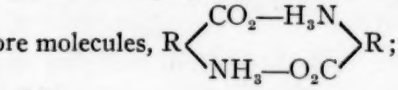
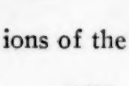
By WILLIAM A. NOYES and RALPH S. POTTER.

(*Read April 24, 1914.*)

During the early years of the development of the theory of valence many chemists held the view that each element has an unvarying valence. The apparent change of valence in nitrogen from ammonia to ammonium salts and in phosphorus from phosphorus trichloride to phosphorus pentachloride was explained by calling the ammonium salts and the pentachloride molecular compounds, as distinguished from ammonia and the trichloride, in which the true valence of the elements was supposed to be shown. This view received support from the dissociation of ammonium salts and of phosphorus pentachloride in the gaseous state. Gradually, with the demonstration that phosphorus pentachloride volatilizes in part unchanged, that phosphorus pentafluoride, PF_5, has a vapor density corresponding to its formula and, in general, that dissociation in the gaseous state does not correspond to any rational distinction between unitary and molecular compounds the view that elements may show a varying valence in their compounds and that nitrogen and phosphorus are sometimes trivalent and sometimes quinquivalent, came to be generally accepted.

More recently Werner[1] has proposed a modified molecular formula for ammonium chloride, $H_3N..HCl$. By this formula he intends to indicate that in the ammonium salts the nitrogen atom retains a normal valence of three but that the nitrogen atom of the ammonia and the hydrogen atom of the hydrochloric acid are held together by secondary ("Neben") valences, the hydrogen and chlorine of the acid retaining essentially the same relation to each other as in the free acid.

[1] See "Neuere Anschauungen auf dem Gebiet der anorganischen Chemie," p. 96 (1905).

An amino acid may, theoretically, assume in the aqueous solution the following forms: (*a*) the free acid,  ; (*b*) a cyclic salt,  , or according to Werner,  ; (*c*) a bimolecular or polymolecular salt formed by the union of two or more molecules,  ; (*d*) the ions of the acid group, and H^+; (*e*) the ions of the base,  and OH^-; (*f*) the double, amphoteric ion, .[2]

The "inner salt" structure was first proposed by Erlenmeyer and Siegel[3] in 1875. Ten years later Ostwald[4] noticed that solutions of glycocoll, $CH_2NH_2CO_2H$, have a very low molecular conductivity and that this is only slightly increased by dilution. He states that in its behavior it is more like a neutral salt than an acid. In 1891 Marckwald[5] called attention to the fact that amino acids of the aliphatic series react only slowly with the mustard oils, while other primary amines react quite readily. Since the amino acids react easily in alkaline solutions, he held that the acids are, in reality, inner salts. Sakurai[6] attempted to substantiate the "inner salt" structure on the preparation from halogen derivatives of the acids and on the resistance which amino acids offer to the formation of acid chlorides. Walker[7] points out that conductivity determinations tell us very little about the structure of glycocoll but that since the conductivity of phenylglycocoll, $C_6H_5NHCH_2CO_2H$, is greater than that of acetic acid it must contain a carboxyl group which ionizes. Tilden and Forster[8] showed that the amino group of amino acids

[2] "Zwitterion."
[3] *Ann., 176,* 349 (1875).
[4] *J. prakt. Chem., 32,* 369 (1885).
[5] *Ber., 24,* 3278 (1891).
[6] *Proc. Chem. Soc., 10,* No. 138 (1894).
[7] *Proc. Chem. Soc., 10,* No. 139 (1895).
[8] *Chem. News, 71,* 239 (1895).

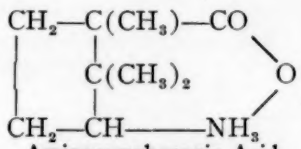
may be replaced by chlorine by the action of nitrosyl chloride and considered this an argument against the inner salt formation. Somewhat later Carrara and Rossi[9] based an argument for the inner salt structure on the conductivity of betaine hydrochloride, $(CH_3)_3NClCH_2CO_2H$. From the values found they considered that the salt was almost completely hydrolyzed to hydrochloric acid and betaine, $(CH_3)_3NCH_2CO$ (O). Winkelblech[10] points out, however, that if betaine hydrochloride is in reality hydrolyzed the conductivity of the solution should be the same as that of the equivalent amount of hydrochloric acid while both Bredig's measurements and those of Carrara and Rossi gave a conductivity scarcely more than one half as great There can be no doubt, of course, that the anhydride of betaine, $(CH_3)_3NCH_2CO_2$, has the structure of a salt, but no one seems to have determined whether this is monomolecular or dimolecular. Our results given below indicate that a solution of an amino acid which gives no inner salt may still contain the acid mostly in the monomolecular form.

Winkleblech[11] discusses the hydrolysis of an amino acid on the basis of conductivity data for weak acids, weak bases and water. It does not seem possible from conductivity data, however, to determine whether the acid is in the form of an inner salt, $R\langle^{CO_2}_{NH_3}\rangle$, in the unionized state, $R\langle^{CO_2H}_{NH_2}\rangle$ or $R\langle^{CO_2H}_{NH_3OH}\rangle$, in the form of the double, amphoteric ion $R\langle^{CO_2^-}_{NH_3^+}\rangle$ or in the form of a bimolecular salt, $R\langle^{CO_2—NH_3}_{NH_3—CO_2}\rangle R$. The hydrogen and hydroxyl ions of the amphoteric form would, of course, combine to form water and if the acid and basic functions were of equal "strength" the solution would react neutral. None of these forms would show any conductivity and while the bimolecular form could be distin-

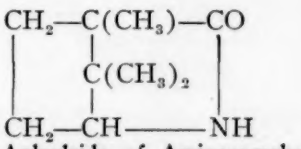
[9] *Atti R. Accad. Lincei* (5), *6*, 208 (1897).
[10] *Z. physik. Chem.*, *36*, 590 (1901).
[11] *Loc. cit.*

guished from the others by a determination of the molecular weight it is not clear how any of the ordinary physical methods could be used to distinguish between the three forms, $R\begin{cases}CO_2\\NH_3\end{cases}$, $R\begin{cases}CO_2H\\NH_3OH\end{cases}$ and $R\begin{cases}CO_2^-\\NH_3^+\end{cases}$. The form $R\begin{cases}CO_2H\\NH_2\end{cases}$ would have a lower molecular weight and might, possibly, be distinguished from the other three by that means. It does not seem to us that the ordinary equations for hydrolysis, which Winkelblech attempts to apply, could be used in a complex case of this sort.

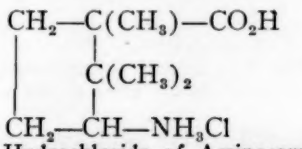
From the above summary it would seem that the evidence with regard to inner salt formation is not altogether satisfactory and light upon the question from an entirely different point of view is welcome. We think that we have secured this from a study of the specific rotations of a series of amino acids derived from camphor. The formulas and names of the compounds are given below. To bring out the relationships more clearly the specific rotations given for the salts are calculated to the basis of one gram of the free acid in 1 c.c. of the solution instead of for one gram of the salt.



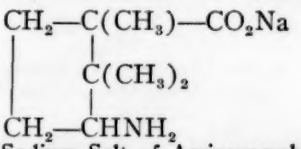
Aminocamphonanic Acid. $(\alpha)_D = -29.2°$.



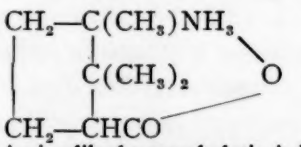
Anhydride of Aminocamphonanic Acid. $(\alpha)_D = -60.5°$.



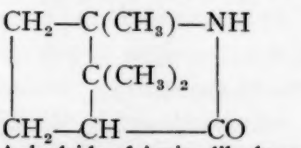
Hydrochloride of Aminocamphonanic Acid. $(\alpha)_D = 25.0°$.



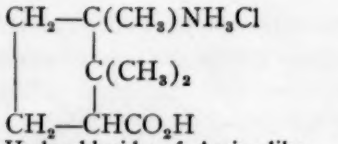
Sodium Salt of Aminocamphonanic Acid. $(\alpha)_D = 52.8°$.

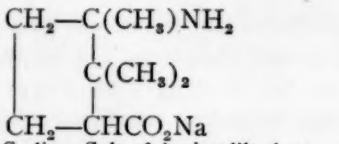


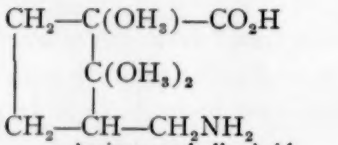
Aminodihydrocampholytic Acid. $(\alpha)_D = 53.7°$.

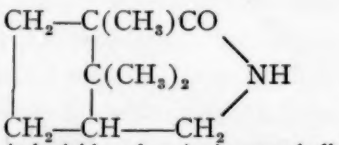


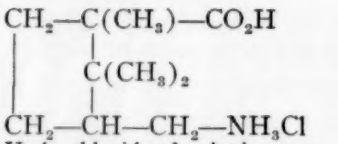
Anhydride of Aminodihydrocampholytic Acid. $(\alpha)_D = 72.8°$.

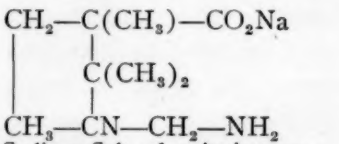

Hydrochloride of Aminodihydrocampholytic Acid.
$(\alpha)_D = 41.3°$.

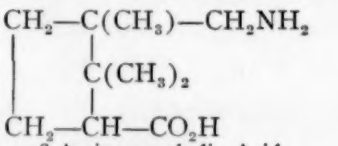

Sodium Salt of Aminodihydrocampholytic Acid.
$(\alpha)_D = 18.3°$.

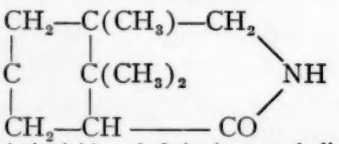

α-Aminocampholic Acid.
$(\alpha)_D = 67°$.

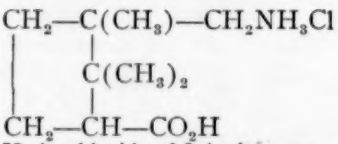

Anhydride of α-Aminocampholic Acid. $(\alpha)_D = -33.9°$.

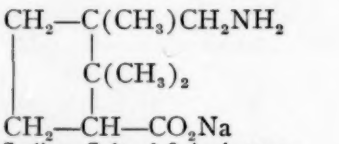

Hydrochloride of α-Aminocampholic Acid. $(\alpha)_D = 44.7°$.


Sodium Salt of α-Aminocampholic Acid. $(\alpha)_D = 62.4°$.


β-Aminocampholic Acid.
$(\alpha)_D = 16.4°$.


Anhydride of β-Aminocampholic Acid. $(\alpha)_D = 66.5°$.


Hydrochloride of β-Aminocampholic Acid. $(\alpha)_D = 41.3°$.

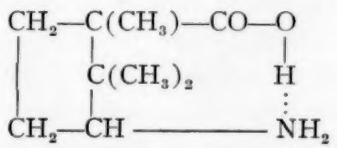

Sodium Salt of β-Aminocampholic Acid. $(\alpha)_D = 14.3°$.

It will be noticed that the aminocamphonanic acid and aminodihydrocampholytic acid are represented as having a cyclic or inner salt structure, while the aminocampholic acids are both represented as having an open structure. The evidence for these structures is based on the specific rotations of the compounds. The rotation of the sodium salt and hydrochloride of aminocamphonanic acid are to the right while that of the anhydride, which is undoubtedly cyclic in

structure, is to the left. The free acid is also left handed, indicating a cyclic structure similar to that of the anhydride. The sodium salt and hydrochloride of aminodihydrocampholytic acid are right handed. The free acid and anhydride are also right handed, but with a considerably increased rotation. The sodium salt and free α-aminocampholic acid are both right handed with rotations closely alike, indicating that each has an open structure, but the anhydride, which certainly has a cyclic structure, is left handed and has a rotation very closely like that of the aminocamphonanic acid, indicating again very clearly that the latter has a cyclic structure and that each *compound contains a cycle of six atoms.* The sodium salt of β-aminocampholic acid and the free acid also correspond closely in rotation, indicating an open structure for both, while the hydrochloride and anhydride have a considerably greater rotation, as is the case with both the free aminodihydrocampholytic acid and its anhydride.

All of these observations are consistent with the hypothesis that aminodihydrocampholytic and aminocamphonanic acid form cyclic salts containing cycles of six atoms, while the aminocampholic acids do not form such salts because, if formed, they would contain cycles of seven atoms. It seems difficult to find any other simple explanation for the observations.

The results also point very strongly to the formula for ammonium salts which represents them as containing quinquivalent nitrogen and against Werner's formula. According to Werner's formula the free aminocamphonanic and aminodihydrocampholytic acids would contain cycles of seven atoms,



Such a formula is quite inconsistent with all that we know about the ease with which rings of five and six atoms are formed and the comparative rarity of seven-atom rings. It is also inconsistent with the close agreement between the rotation of the aminocamphonanic

acid and that of the anhydride of α-aminocampholic acid. We know that the latter compound contains a six-atom ring.

Determinations of the molecular weights in aqueous solutions by the freezing point method have shown that all four of the amino acids are monomolecular in such solutions.

The experimental details of the investigation will be published in the *Journal of the American Chemical Society*.

SOME FURTHER CONSIDERATIONS IN THE DEVELOPMENT OF THE ELECTRON CONCEPTION OF VALENCE.

By K. GEORGE FALK.

(*Read April 24, 1914.*)

The electron conception of valence is based upon the view that when two atoms combine, one becomes charged positively and the other negatively. According to J. J. Thomson,[1] the union of two atoms is brought about by the transfer of a negatively charged corpuscle from one atom to the other; the atom losing the corpuscle becoming charged positively, the one gaining the corpuscle, charged negatively. In order to represent graphically the linkings between atoms the lines or dots which represent the bonds ordinarily are replaced by arrows in the electronic considerations, the head of the arrow indicating the direction in which the corpuscle is assumed to be transferred in the production of the chemical bond.

In every discussion of valence, it is necessary to consider the limitations of the problem. Valence is a number. The valence of an atom shows the number of corpuscles or negative electrons gained or lost by that atom in forming chemical bonds. In slightly different terms, the valence of an atom shows the number of atoms (or groups of atoms) held in combination by that atom when the hydrogen atom as it exists in most of its compounds is taken as the positive unit. Valence may be likened to the capacity factor in energy considerations.[2] Like the capacity factor, it is denoted by a definite number, and while this number may vary under different conditions, a quantity or number of atoms (or combining weights) held by an atom (or combining weight) of the element in question is always meant.

[1] "The Corpuscular Theory of Matter," pp. 138–9 (1907).

[2] Cf. S. L. Bigelow, "Theoretical and Physical Chemistry," p. 80 (1913).

In the same way, the chemical affinity between two combining atoms would correspond to the intensity factor in chemical energy. This chemical affinity can be measured quantitatively only by the change in free energy of the reaction in question. Stability relationships, or chemical affinity discussions, do not enter directly into valence questions, although the existence of substances is controlled entirely by these. The separation of these two problems, valence and chemical affinity, makes it clear that while a great number of substances may be predicted from a consideration of valence structures alone, questions of chemical affinity, or relative stability, limits the number of these substances which are actually known or may be prepared.

As a result of the comparative study of large numbers of compounds, it has become possible to say which would probably exist under ordinary conditions and whether some would react more rapidly than others. These qualitative factors do not give any information concerning the real quantitative measures of relative stability.

Valence is therefore essentially a classifying principle. While it is based directly upon and derived from the atomic theory, it may also be used without considering atoms. The conception of atoms and molecules is based upon the experimental laws of definite and multiple proportions, and if, instead of atoms and molecules, combining and formula weights are used, the same relations will be found, although perhaps not pictured as readily.

The most important feature of the present development is that in speaking of the valence of an element, it is not sufficient to give a number. It is just as important to state whether this number is positive or negative, as the valence or the number of unit atoms or groups held in combination involves also the question whether these are electropositive or electronegative. This is brought out clearly in the Periodic System of Mendeléeff, especially for Groups 4 to 7, where the types of combination with hydrogen and with oxygen represent the maximum negative and positive valences of the elements of these groups.

Since the valence of an atom may be positive or negative depend-

ing upon the loss or gain of corpuscles, the knowledge of the electrical state of an atom in a compound is of importance. Ionization in solution is an invaluable aid in determining the distribution of these charges. Dissolving a substance does not produce electric charges on atoms but only makes these charges manifest to certain experimental methods. These are, in fact, the only direct experimental methods for determining valence. For substances which ordinarily do not ionize, a knowledge of the distribution of the charges is also important. This question was taken up for organic compounds and reactions by Professor J. M. Nelson, of Columbia University and the speaker in a number of papers.[3] It was shown that satisfactory classifications could be developed with the electron conception of valence alone, but that the use of both polar and non-polar valences leads to contradictions in reactions which are fundamentally similar.

With compounds which do not ionize, the Periodic System serves in a general way as a guide for developing valence structures in which relative positive and negative properties of the different atoms are involved. When two atoms are united by a single bond (one unit of valence), where one corpuscle is transferred in the production of the bond, there is ordinarily no question of the direction of transference of this corpuscle, that is to say, which element is positive and which is negative. If isomers exist, the difference in the relative affinity of the atoms for the negative corpuscle may be small (as in iodine monochloride) and the less stable modification may possess the structure in which the corpuscle is transferred in the opposite direction from that of the stable modification.

Some interesting questions are raised when the double bond is considered from the electronic point of view. As used in the past, the justification for the double bond lies in the desire to maintain consistently, constant values for the valence of certain atoms. Practical work during the past fifty years has borne out within certain limits the usefulness of this conception. Before discussing the significance of the double bond with the newer ideas of valence, some general

[3] *Jour. Amer. Chem. Soc.*, *32*, 1167 (1910); *33*, 440 (1911); *35*, 1810 (1913); *36*, 209 (1914).

facts must be mentioned. A single bond between two atoms gives no information as to the stability of the union between these atoms. A double bond between two atoms cannot give any more information with regard to the stability of the linking. Qualitatively it has been found that the rate of reaction for compounds containing double bonds is greater in some ways than the rate for compounds containing single bonds, and that with certain reagents decomposition at the double bond occurs more rapidly than at other parts of the molecule, but this is manifestly different from a discussion of true stabilities of compounds. Reaction velocities bear no simple relation to stabilities of substances and "reactivity" as used in organic chemistry very often refers only to these reaction velocities. The double bond in the ordinary language signifies two units of valences just as the single bond denotes one unit of valence, and in this sense, the only permissible one, the representation of a double bond by two lines is a correct picture of the linking when one line is used for the single bond.

When two atoms of elements which differ very markedly in electrochemical properties are combined by a double bond, one of these atoms may be considered to be electropositive and the other electronegative. In valence terms, an atom of one of these elements has given up two negative electrons to the atom of the other to form the double bond, the former becoming positive, the latter negative. These cases are as simple as those in which only single bonds are involved. Two units of valence are used in each linking with the result that the valence of one of the atoms is $+2$ and of the other -2 due to the double bond.

If two atoms of elements which do not differ much in electrochemical properties are combined by a double bond, the possible relations from the electronic point of view are somewhat more complex. On the other hand, explanations of reactions and classifications of isomers are afforded which are not possible with the view of the double bond in which electrons are not considered.

For the present purpose, it will be sufficient to outline some of the relations. The substances to be considered include mainly the compounds of carbon with double bonds between two carbon atoms

or between one carbon atom and an atom of some other element such as oxygen or nitrogen. In order to illustrate the present discussion with a definite case, a compound containing a double bond between two carbon atoms in which the other bonds are combined with similar groups, may be chosen. Two possible arrangements suggest themselves with regard to the directions in which the corpuscles may be transferred to form the double bond. One carbon atom may lose two corpuscles and the other may gain two in the formation of the double bond. In this case, the valence of the first carbon atom due to the double bond is $+2$, of the second -2. The other possibility involves the gain and loss of one corpuscle by each of the carbon atoms in forming the double bond. In this case, the valence of each carbon atom due to the double bond will be $-1+1$. Since the oxidation of an atom is defined as a decrease of the negative charge or number of corpuscles, and reduction as a decrease of the positive charge, these atoms united by the double bond would be present in different states of oxidation in the different isomeric substances. It is evident therefore, that, with the electron conception, the double bond may show different reactions with various reagents depending upon the directions of the valences of the double bond or the state of oxidation of the atoms united by the double bond. Isomeric substances might exist in which the isomerism would be due to the different directions of the valences of the double bond. This subject has been discussed at some length in previous papers.[4]

Similar relations should be expected to hold with compounds containing a triple bond. While not as much work has been recorded in the literature for substances of this nature, it has been possible, with the electron conception of valence, to explain some reactions of compounds containing triple bonds much more satisfactorily than with the older valence view.

The general view of valence is that of a classification of chemical compounds and reactions. Since the introduction of the electronic nature of valence into all branches of chemistry widens and

[4] Cf. *S. of M. Quarterly, 30,* 179 (1909); *Jour. Amer. Chem. Soc., 32,* 1167 (1910).

extends the classification, and since much of the classification depends upon a number of correlated facts and relations and not upon single crucial and well-defined experiments, it may be expected that some of the formulas advanced and explanations of reactions offered at the present time will be subject to change. Caution must continually be exercised against reading into valence structures ideas which are foreign to valence. A limitation of the questions discussed to the phenomena which may rightly be included would obviate much confusion and bring valence relations into clearer light.

HARRIMAN RESEARCH LABORATORY,
ROOSEVELT HOSPITAL, NEW YORK.

HEWETTITE, METAHEWETTITE AND PASCOITE, HYDROUS CALCIUM VANADATES.

By W. F. HILLEBRAND, H. E. MERWIN and FRED E. WRIGHT.

Introduction.

(*Read April 25, 1914.*)

Some years ago, Mr. D. Foster Hewett, in a paper[1] on the remarkable vanadium locality of Minasragra, Peru, described briefly certain oxidation products of the vanadium sulphide ore, patronite, which he was inclined to regard as vanadic acids, although the opinion of one of us (H.), based on preliminary analyses, was that two of the minerals were calcium vanadates.

Several years later the chief constituent of a certain red ore of vanadium from Paradox Valley, Montrose County, Colorado, was identified (H.) as a calcium vanadate, seemingly identical with one of those from Peru. Since then this red ore has been found over a wide area, extending into Utah. A good deal of additional chemical work has been done intermittently during the past three years upon material of both occurrences and it has been studied microscopically. It has developed that, although the minerals are deceptively alike in appearance and general behavior and have the same empirical formula, they seem to be specifically distinct, are probably in fact isomers. The Peruvian mineral, the first known and studied, we are pleased to name *hewettite*, after Mr. D. Foster Hewett, now of the U. S. Geological Survey, who has done so much to make the Minasragra occurrence known. Its isomer may appropriately be called *metahewettite*. It is probable that hewettite occurs also in Paradox Valley (see under metahewettite, pp. 37–38).

The detailed results of the several authors' work are submitted in the following pages, a preliminary announcement having appeared in the *Jour. Washington Acad. of Sci., 3,* 157, 1913.

[1] *Trans. Am. Inst. Min. Eng., 40,* 291, 1909.

Hewettite.

Hewettite was rather abundant at the time of Mr. Hewett's visit to Minasragra. It is wholly of superficial occurrence, derived by oxidation from the vanadium sulphide, patronite. The principal specimen examined by us was a lump about the size of a small apple.

In the lumpy aggregates of pure mineral hewettite is deep red (mahogany red)[2] with a somewhat silky luster. Under the microscope the needles measure usually less than 0.01 mm. in width and 0.2 mm. in length. The extinction is parallel. The refractive indices measured by immersion methods could be only approximately found because of the extreme thinness of single blades and the lack of entire parallelism of the blades in a group. Also, β and γ were so high that slight heating was necessary to embed the mineral in the standard refractive media, thus causing expulsion of an unknown amount of water. For Li-light $\alpha = 1.77$, $\beta = 2.18$, $\gamma =$ about 2.35 to 2.4. Elongation is parallel to γ. Pleochroism is strong; γ dark red, α and β very light orange-yellow. The mineral is probably orthorhombic. A determination of density, made by Mr. E. S. Larsen, on air-dried material gave a value 2.618. A subsequent determination by one of the authors (M.), using material containing 9 molecules of water, gave 2.554. The apparent discrepancy between these two determinations is explained by the fact that the material used by Mr. Larsen was partially dehydrated.

The mineral when heated passes through various color changes (see p. 46) and melts readily, forming a dark red liquid; it is slightly soluble in water.

The composition of an almost pure specimen, on which the foregoing optical examination was made, is given under analysis 1 (p. 40). Analysis I*a*, in the footnote on pp. 40–41 (quoted by Mr. Hewett, loc. cit., p. 311), represents a lump of ore showing little evidences of crystallization but otherwise resembling closely the better specimen, although the microscope shows it to be far from homogeneous. In spite of the similarity in appearance of the two specimens, the

[2] The specific color terms used in these descriptions are based on comparisons with Ridgway's standards. See "Color Standards and Color Nomenclature," Robert Ridgway, Washington, D. C., 1912.

analyses show quite different percentages of lime. This is not surprising if we conceive that the vanadium of the patronite has become oxidized to a polybasic acid of quinquivalent vandium, which then was gradually neutralized by calcium. It seems reasonable to expect that ores of all gradations occur from the sulphide patronite to the half neutralized salt hewettite and finally to the fully neutralized salt represented by pascoite (p. 49). Indeed, such intermediate stages are probably represented by a number of the specimens brought from Peru by Mr. Hewett and of which a few analyses are given in his paper. These ores are, for the most part, microcrystalline, though some show distinct evidences of crystallization, as Mr. Hewett pointed out. They are of varying colors, from red through greenish to the black of the original patronite. Some of the specimens are not fully oxidized[3] but contain vanadium in a lower state of oxidation than corresponds to quinquivalency, and even free sulphur. Some of them are characterized by high iron content and relative freedom from lime, as shown by the following analysis,[4] which seems to represent essentially a ferric vanadate.

V_2O_5	57.3
V_2O_4	4.8
MoO_3	3.3
Fe_2O_3	19.6
TiO_2	.1
SiO_2	.6
CaO	.7
H_2O	13.9
	100.3

Molybdenum is naturally a characteristic component of the oxidation products of patronite since it occurs in the patronite ore.

Metahewettite.

Unlike the Peruvian mineral, the North American vanadate is an impregnation in sandstone, generally coating the sandstone grains, sometimes filling cavities and crevices. The specimens are almost

[3] Analysis I indicates that oxidation is not yet quite complete in our best specimen.

[4] Made in 1907 in the laboratory of the U. S. Geological Survey and quoted by Mr. Hewett, *loc. cit.*, p. 311.

PROC. AMER. PHIL. SOC., LIII. 213 C, PRINTED JUNE 17, 1914.

always friable, some falling to powder. Gypsum frequently accompanies the metahewettite and often encloses it, producing then the appearance of a distinctly crystallized red mineral. When pure the powder is dark red; gypsum present lightens the color. Even when free from gypsum the red vanadate is almost always associated with other minerals, partly residuals from the impregnated sandstone, although occasionally almost pure material is found, like that represented by analysis II (p. 40). The impurities interfered greatly at first with the precise determination of the composition of the vanadate, particularly as to its water content. For this reason no quantitative analysis of the mineral from Paradox Valley is given, although an abundance of the ore was at our disposal and one or more analyses of it were made before purer material, from Thompson's, in eastern Utah, was obtained. These analyses made evident, however, the chemical identity of the Paradox and Thompson's minerals.

The ore at our disposal from Thompson's differs somewhat from that of Paradox Valley by a greater variation in its shades of red, some of these being very bright in contrast with the usually duller shades of the ore from Paradox.[5] There are also associated with it at least two interesting minerals, both of which were also noticed later in ore from Paradox and the Henry Mountains.

One of these, gray in color, is a hydrous silicate of aluminum, trivalent vanadium and potassium. It is no doubt the same silicate that was first noted by one of us (H.) in carnotite ores and seems to be a constant associate of all the uranium and vanadium ores of western Colorado and eastern Utah, in some places constituting the chief vanadiferous component of the ore. In the ore from Thompson's it forms soft patches throughout the red mass, some of which are of sufficient size to permit of separation in a fairly pure state.[6] Per-

[5] Ores of deep color have been found recently by Mr. Frank L. Hess, of the U. S. Geological Survey, in the Henry Mountains, Utah.

[6] The gray mineral accompanying metahewettite occurs in firm granules consisting of aggregates of very minute doubly refracting particles which cannot be isolated for microscopic study. The refractive indices of aggregates from various portions of the ore varied between 1.59 and 1.64, apparently indicating differences in composition. Some larger lath-shaped particles

haps the greenish tints of some of the Paradox ore specimens are caused by this or another related silicate, like the roscoelite from Placerville, Colorado.[7]

The second characteristic mineral is selenium, in amount up to one per cent. of the ore from Thompson's. It seems to be included as specks in the gray silicate. It is entirely absent from the specimens of Paradox ore first obtained, but is present in ore from the Henry Mountains, Utah, and in some small specimens seen recently that were said to come from Paradox Valley. The presence of selenium can be detected by heating the ore in a glass tube closed at one end, when a red sublimate (sometimes accompanied by a white one of selenium dioxide) appears when most of the water has escaped. The fact that the free element appears as a sublimate does not by itself prove the existence of the selenium in the free state in the ore, for there was enough organic matter present, in a state invisible to the eye, to reduce an oxygenated compound of selenium if present. But the weight of evidence points to its presence in the elemental state and not as a selenide or oxygenated compound.[8] No connection was observed between the presence of the selenium and the bright red color of some specimens of the ore. The differences in shades of red are attributed to differences in physical condition of the metahewettite and to the effect of associated minerals.

were observed, possibly pseudomorphs, containing abundant dark inclusions of more or less prismatic shape arranged parallel to the laths. These laths are aggregates, but portions of them seem to have a definite orientation with respect to the outlines, extinguishing parallel and having γ parallel to their lengths. The inclusions were selenium and bituminous matter. This gray material is probably not roscoelite. For purpose of comparison a study was made of the properties of the roscoelite from Placerville, California, probably identical with that previously analyzed by Hillebrand (*Am. J. Sci.*, 7, 351, 1899; Bull. U. S. Geol. Survey, No. 167, p. 70, 1900).

Optical properties of roscoelite: Color, deep green with almost metallic luster. 2E variable between 60° and 75° or more. Optical character—. $\gamma = 1.680$–1.685, $\beta = 1.675$–1.680.

[7] Hillebrand and Ransome, *Am. J. Sci.*, *10*, 120, 1900, Bull. U. S. Geol. Survey, No. 262, p. 18, 1905.

[8] The mineral presents deep red transparent prisms, up to 0.05 mm. long, showing parallel extinction. This characterization fits one of the known forms of selenium. Sublimation tests on a few specks indicated free selenium. It seems to be insoluble in carbon bisulphide. So far as known this is the first established occurrence of elemental selenium in nature.

Nearly all of our ore specimens from Paradox Valley, Thompson's and the Henry Mountains were free from uranium minerals, but the complexity of the metahewettite ores under special conditions is well illustrated by a very small specimen from Paradox. In addition to constituents indistinguishable to the eye, this showed in juxtaposition and much commingled, metahewettite, carnotite, a brownish material rich in uranium and resembling some forms of ferric phosphate, and jet black, lustrous bituminous or coaly matter. This last, if uraniferous, is perhaps the unnamed mixture of which a preliminary notice by Karl Kithil appeared in *Science, 38*, 625, 1913.

Fortunately several small lumps of very pure material from Thompson's were found. This material gave only a very faint reaction for selenium and was otherwise almost free from contamination. Upon it analysis II. is based.

Metahewettite crystallizing with 9 molecules of water occurs in two typical habits with intermediate forms. The purest material from Thompson's is a feebly lustrous, loose, earthy powder; that from Paradox Valley appears chiefly in compact aggregates of separable, shining blades, though the earthy variety also is found here. Both are deep red, but on account of the larger size of its bright reflecting surfaces the bladed variety appears lighter colored. When powdered the bladed variety is claret brown, the earthy variety is dark maroon. The color of the ores containing the mineral varies greatly because of admixed minerals; furthermore, variation of the water content of the mineral produces changes in color (see p. 46).

Microscopically the earthy variety consists of minute sharply bounded tables about .04 mm. long, piled in subparallel groups. The outlines and optical properties indicate orthorhombic symmetry. The compact variety consists of plates like those in the earthy variety, closely joined in parallel or radiating, more fibrous aggregates. The optical properties are more easily determined on these large aggregates. Pleochroism is strong in groups seen edgewise, but is scarcely noticeable in the plane of the tables. α is light orange-yellow, β deep red, γ deeper red. Two optic axes barely come into the field of a No. 12 objective over a condenser immersed in oil. 2E thus measured is about 135°. The plane of the optic axes is parallel to the

elongation. The refractive indices α and β were determined with difficulty. α was obtained from groups of crystals seen edgewise; β is so high that it could barely be matched without heating by immersion in a mixture of tin iodide, methylene iodide and the compound of arsenic sulphide and methylene iodide. γ could not be obtained except after expelling water from the mineral by heat.

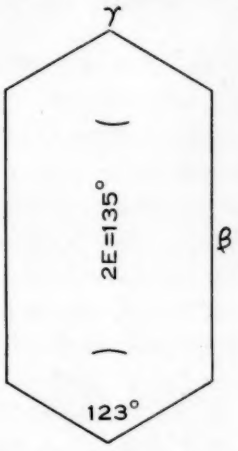


FIG. 1. Optical orientation of metahewettite.

For Li-light $\alpha = 1.70$, $\beta = 2.10$ and $2V$ (calculated) $= 52°$. α is the acute bisectrix. No measurable differences between the optical properties of specimens from different localities were found. Fig. 1 shows the optical and crystallographic relations. The axial ratio $a : b = .54 : 1$.

The density of the mineral containing 9 molecules of water—determined in xylol—is 2.511; after the loss of 6 molecules of water it is 2.942. The loss of this water is not accompanied by any perceptible breaking down of crystal structure. The same fact was observed with hewettite. Like hewettite, metahewettite melts readily and is slightly soluble in water. The solubility, slight as it is, affords a means, by the use of much boiling water, of separating from metahewettite the associated minerals mentioned on pages 34 and 35.

Differing strikingly in external aspect from the ores described

above is a single specimen from Paradox Valley, shown us by Mr. Frank L. Hess, of the U. S. Geological Survey. This consists of a single bundle of interwoven fibers implanted on a layer of crystallized gypsum. This specimen, on account of its markedly fibrous structure, bears a closer resemblance to the best of the hewettite from Peru than does the mineral described above. Indeed, optical evidence shows that it is hewettite.

Analyses of Hewettite and Metahewettite, and Their Discussion.

Long after the first analysis of hewettite was made, it was found that both it and metahewettite are extremely sensitive to atmospheric changes in humidity, especially within a certain narrow range. It was therefore essential that they be brought always to the same definite state of saturation with respect to water.[9] It was found by experiment that this condition could be satisfactorily attained by exposing the mineral powder at a definite temperature over sulphuric acid of vapor tension near that of pure water until equilibrium was established. The strength of acid over which the mineral was placed

[9] Failure to observe this precaution may lead to serious error in establishing a probable formula for minerals with variable water content. For instance, if, as with hewettite and metahewettite, the mineral is analyzed in air-dry condition when the air humidity is high, a very different result will be obtained than when the air is dry. The variations in moisture content of these minerals when left exposed to the air may vary 8 or 9 per cent. between September and December in Washington. This is not efflorescence as usually understood, for although the reaction reverses itself with return of humidity the loss of water is not accompanied by breaking down of the crystal structure.

Calcio-carnotite from Colorado (the tyuyamayunite (?) of Nenadkevich) and probably also the original carnotite (essentially the potassium salt) show similar wide differences in water content at different seasons of the year. This may be and probably is true also of other minerals. If so, an explanation is afforded of some of the conflicting statements of different analysts in regard to the water content of certain minerals.

It is, further, important to make a series of exposures over sulphuric acid of increasing concentrations, up to the maximum, at a fixed temperature, and then to carry out tests at rapidly increasing temperatures, in order to detect, by the losses at each step, the number of hydrates that may exist, and also, if possible, what proportion of the water may be differentiated from water of crystallization or of absorption. The curves resulting from such tests with these minerals are shown in Fig. 2.

preparatory to analysis was chosen after a preliminary trial had shown about the range of highest vapor pressure over which little change in water content took place. Sulphuric acid of sp. g. about 1.105 (15°) was found to give the desired vapor pressure, about nine tenths that of saturation.[10] Two different temperatures were employed for establishing equilibrium over this acid in a thermostat, namely, 25° in winter and 35° in summer (temperatures most easily maintained).

At 25° the water-vapor tension of this acid is about 21.8 mm. or 2 mm. less than that of pure water at the same temperature, and 38.8 mm. at 35°, or about 3.5 mm. less than that of pure water. Upon material thus brought to a definite water content the analyses were made. One portion was used for studying the course of dehydration, first by exposing the mineral at 25° (or 35°) until equilibrium was reached, successively over sulphuric acid of concentrations that corresponded to lowering of the vapor tension by tenths approximately and finally over phosphorus pentoxide.[11] With the

[10] The tables of Domke and Bein were used in this connection (*Z. anorg. Chem.*, *43*, 176, 1905).

[11] The value of conclusions deducible from such a series of fractionations on minerals of the kind in question depends on careful observance of certain precautions. In the first place, the mistake must not be made, as in the present case, of using different temperatures for the initial saturation. It was not expected that our tests would extend from winter into summer, as they did at intervals, thus necessitating the use of two temperatures. Of course, the vapor tension of an acid of given strength is markedly greater at 35° than at 25°. The initial water content of the mineral may therefore differ and the results of tests started at the two temperatures not be strictly comparable. For, although the mineral is under a greater water vapor tension of the acid at 35° than at 25° and might therefore perhaps take up more water than at the lower temperature, it is probable that the higher temperature will have its effect on the mineral also but in a quite indeterminate degree (see Fig. 2 and p. 44 of text, for an instructive illustration of one effect of temperature differences, as shown by curves I. and II.). In the second place, at the conclusion of any one test of a series the mineral must not be allowed to cool in the desiccator after removal of the latter from the thermostat, but must be taken out of it at once and inserted quickly into a capped weighing vessel before it has time to cool, for if allowed to cool before removal the final condition will approach that of room temperature and not be that of the experiment. Moisture may be condensed on the mineral and its containing vessel and the former may perhaps reabsorb water in

lower vapor pressures a vacuum was employed. The loss observed at the end of the last exposure was practically identical with that occasioned by a temperature of 100° for a few hours in dry air. Then the fractionation was continued at temperatures above 100°. The results of some of these tests will be given later (pp. 44–45).

The following table of analyses does not show all the determinations that were made. In fact, the first complete analysis of hewettite is omitted, because it was made before the need for bringing the mineral to a definite saturation as to water content was realized. That analysis confirmed, however, in all other essentials than water, the data contained in analysis I. below.

ANALYSES OF HEWETTITE AND METAHEWETTITE.

	Hewettite,[12] Peru, in Equilibrium with Water-vapor Tension of 21.8 mm. at 25°. I.	Metahewettite, Utah, in Equilibrium with Water-vapor Tension of 38.8 mm. at 35°. II.	
V_2O_5	68.19	70.01	
V_2O_4	1.21	——	
V_2O_3	——	.35	
MoO_3	1.56	.13	
CaO	7.38	7.25	7.25
MgO	none	.03	
K_2O	none	.09	
Na_2O	.15	.08	
H_2O (total)	21.33	21.30	(mean of 21.24, 21.31 and 21.34)
Fe_2O_3, etc.	.11	.19	
SiO_2	——	.80	
Insol.	.17		
	100.10	100.23	

Trace Li in I., none in II. A very little Cl in I. and II. Trace P_2O_5 and Se in II. No Ba or Sr found in either. V_2O_4 and V_2O_3 assumed, their amounts measured by consumption of permanganate when the mineral was dissolved in dilute sulphuric acid (see discussion of molecular ratios, p. 41).

addition to that condensed on its surface. In the third place, in cases when a vacuum is employed this should be relieved by air bubbling through acid of the same strength and temperature as that in the desiccator, if possible while the latter is still in the thermostat.

[12] An earlier analysis of more compact material from Peru, devoid of crystalline appearance, gave the following results:

Molecular Ratios.

In calculating molecular ratios for the minerals one is confronted with difficulties arising from the presence of vanadium of lower valence than 5, of molybdenum, and of small amounts of constituents other than calcium. These different problems will be taken up in order.

Vanadium.—In hewettite the presence of vanadium in the quadrivalent state may be regarded as probable in view of its existence in many of the specimens representing much less complete oxidation of patronite. In these cases it may be assumed with considerable probability that there exist vanadyl-vanadic oxides or salts, since artificial compounds of the kind are known. If a compound of this nature exists in hewettite, it demands a portion of the V_2O_5, and, if hydrated, a considerable percentage of water.

In metahewettite, however, there is great reason to believe that the vanadium of lower valence is trivalent. That it exists as a constituent of a silicate containing also aluminum and potassium was pointed out on p. 34. It will be so regarded. This stand is taken with full knowledge that a characteristic black ore from Paradox contains much of its vanadium in the quadrivalent state, as mentioned in our preliminary paper.[13]

Molybdenum.—The presence of molybdenum hampers some-

	Ia. Per Cent.	Mol. Ratio,
V_2O_5	66.8	4.79
V_2O_4	.7	
MoO_3	2.8	.26
CaO	4.3	1.00
H_2O (100° —)	13.9	10.03
H_2O (100° +)	6.9	4.97
Fe_2O_3, etc.	3.3	
SiO_2	1.2	
	99.9	

This analysis is given chiefly to show how deceptive the evidence afforded by sharp molecular ratios may be, for microscopical examination showed the material to be very far from pure. It also serves to show what different compositions similar appearing materials may have.

[13] *Jour. Washington Acad. Sci., 3,* 158, 1913.

what the drawing of conclusions as to the formulas assignable to the vanadates, since we know nothing positive as to its chemical condition and whether foreign to the vanadate molecule or a part of it, nor, if foreign what part if any of the water is to be assigned to it. So much may, however, be affirmed with positiveness from careful microscopical examination, that the sample of hewettite analyzed represents essentially a single homogeneous mineral and not a mechanical mixture of different minerals. This belief is supported by the behavior of the mineral when it is gradually brought into complete solution by successive treatments with much hot water, for the solution of the molybdenum keeps pace with that of the vanadium. The molybdenum may perhaps best be considered as forming calcium molybdate which is held in solid solution. This assumption has been made because it seems called for by the varying proportions of molybdenum in different specimens and by the difficulty of deducing a probable formula under any other assumption.

Other Constituents.—The absence of any acidic constituent in hewettite to offset the sodium forces us to group this with the calcium as part of the vanadate molecule, unless perchance there be an admixture of a vanadyl-vanadate (see p. 41). In metahewettite the potassium may be referred with a high degree of probability to the silicate of which mention has been made. This silicate requires a small part of the water. There is no evidence of such a silicate in the Peruvian mineral. The sodium and magnesium of metahewettite are not accounted for, but in part at least may belong to the silicate mentioned or to another, except in so far as the chlorine present in small amount may claim some of the sodium (also in hewettite). The amounts reported for sodium may be subject to considerable error in both analyses, and if in error are too high. The iron oxide is no doubt admixed.

After deducting MoO_3 and its equivalent of CaO and neglecting V_2O_4, V_2O_3 and all other minor constituents except Na_2O in I., the molecular ratios deducible with employment of the 1913 atomic weights are:

	I.	II.
V_2O_5	3.06	3.00
CaO	1.00	1.00
H_2O	9.61	9.20

The value for V_2O_5 in I. becomes 3.00 if enough is deducted to form an equi-molecular compound with the V_2O_4.

Before discussing the formulas of the minerals we must present a number of considerations bearing on their specific differentiation, in the course of which certain experimental data essential to a proper understanding of the subject will be given.

Evidences of Specific Distinction of Hewettite and Metahewettite.

The evidence which impels us to give different names to the Peruvian and North American minerals, in spite of the fact that they seem to have the same empirical formula, will now be set forth.

Optical and Crystallographic Differences.—Metahewettite is

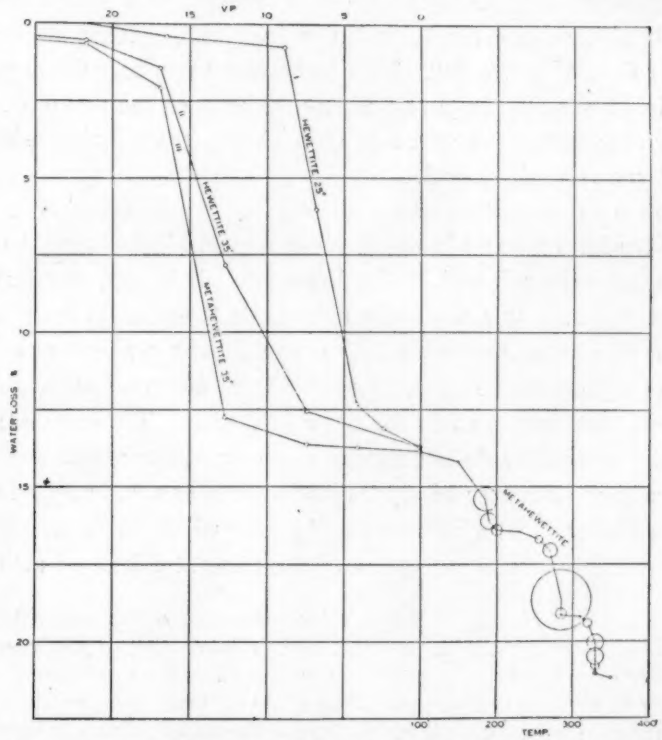


Fig. 2. Showing the course of dehydration of hewettite and metahewettite over sulphuric acid of different concentrations at room temperatures, and of metahewettite at temperatures above 100°.

darker red than hewettite and has two directions of strong absorption for light, while hewettite has only one. Metahewettite crystallizes in well-defined tabular forms or broad blades, hewettite in slender blades.

Loss of Water at Room Temperature.—There is a real difference in the behavior of the two minerals during progressive dehydration over sulphuric acid. Reference to Fig. 2 (curve III.) shows that at 35° metahewettite loses 10.65 per cent. of water in one drop from 17 mm. to 12.7 mm. vapor tension, nearly as much as hewettite loses in the two stages from 17 mm. to 7.4 mm. (curve II.). The points of observation shown in the several curves of the figure in almost all instances represent 24-hour exposures and are approximately equilibrium points. It was unexpectedly found that equilibrium at these points was practically attained in one day, for even after several days' exposure the losses in weight were seldom appreciably greater than after one day. This fact seems to support the argument that the curves indicate a real difference between the two minerals.[14] The curves shown are as a rule quite closely reproducible with different samples of mineral.

The very important effect of varying temperature at a fixed vapor tension of water in the desiccator is strikingly brought out by comparing curves I. and II. for hewettite. It is seen, for instance, that at 35° and 12.7 mm. vapor pressure the mineral loses nearly 8 per cent. of water against a little over half of 1 per cent. at 25° and the same pressure. Again, curve I. shows an equilibrium point at 6.05 per cent. water, curve II. at 7.85 per cent. To the question of why the two points do not appear in the same horizontal line if we are dealing with water of crystallization, two answers suggest themselves: Either (1) an intermediate hydrate which forms at 25° may not have appeared at 35° or, (2) the concentrations of acid used

[14] The lines connecting points of observation in the figure do not signify that the rate of loss was uniform for each unit-lowering of vapor pressure for the interval between two points. The actual loss for each interval would probably be more correctly expressed by a vertical line corresponding to some vapor pressure intermediate between those of the actual points of observation. Just where this line should fall might be determined by making observations with acids of smaller variation of vapor pressures and allowing more time for the attainment of equilibrium.

were not sufficiently close together to indicate all the hydrates formed.

Loss of Water When Heated Above 100°.—Other differences become apparent when the minerals are heated up to the point of losing the last molecule of water. The experiments showing this were made at the geophysical laboratory, the heating being done in an oil or a nitrate bath, both in an open tube and in tubes which were evacuated from time to time, and the loss determined by weighing the tubes.

Metahewettite, after losing 13.8 per cent. (six molecules) of water over strong sulphuric acid, was first heated gradually from 100° up to 350° during one hour with weighings at frequent intervals. Water was expelled abundantly at three stages near the temperatures 185°, 275°, and 340°. The loss at each stage as determined by these weighings and by repeating the heatings and weighings on another sample at favorable temperatures, was found to correspond to about 2.3 per cent., or one molecule of water at each stage[15] (see Fig. 2, lower portion). The sizes of the circles represent the loss of water at each heating divided by the time of the heating. This is the rate of loss of the water.

Hewettite, on the other hand, exhibits only one well-marked loss in weight when heated rapidly. Near 250° a loss corresponding to two molecules of water was observed. Near 300° and 350° there are evidences of increased rate of loss if the temperature is increased rapidly in 30 minutes. These losses correspond to one-half molecule each. These data are not shown in the figures.

[15] This method of rapid heating and frequent weighings affords surer indications of the existence of water in different stoichiometrical proportions than that of holding the mineral in dry air for a long time at successively increasing temperatures. By either method a rate of dehydration, not a condition of equilibrium, is measured. Fig. 2 shows two distinct breaks in the rate of water loss for metahewettite when it is heated rapidly as described in the text. The curves plotted for 5-hour periods of heating in a dry-air current (not shown in the figure) are without distinct breaks.

The temperatures at which water escapes most rapidly bear little relation to those employed in slow heating. Long heating results in a higher loss at a given temperature, and in complete dehydration at a much lower temperature, than very rapid heating.

Change of Color Due to Heating.—Metahewettite, dark maroon in color at first, becomes progressively darker red till the last molecule of water begins to escape, then the color becomes gradually lighter and finally yellow-brown. Concurrent with the last change is a breaking up of each crystal into a crystalline aggregate which retains the form of the original crystal.

Hewettite also darkens when water is lost. At the start, with nine molecules of water, it is mahogany red. The hydrate with three molecules is between carob-brown and liver-brown, the compound with one molecule chestnut-brown, and the anhydrous powder medal-bronze. No breaking down of structure was observed till the last molecule of water was lost.

Changes in Weight and Color after Dehydration.—The powders of both minerals were exposed after complete dehydration to sulphuric acid of sp. g. 1.10. Metahewettite from Utah, after dehydration by heating in air, regained its original weight and color in a few days, but after treatment under greatly reduced pressure the color was not restored, even after moistening, although the observed loss of weight had been the same in both cases. Hewettite, on the other hand, after heating to only 270° and while it still retained one molecule of water, neither regained its original color nor quite its original weight when placed in moist air. In one experiment, after heating to 350° and losing 20.75 per cent. in weight, it regained 19 per cent., but on again fractionating over the acids first used the rate of loss and the amount lost at each step were markedly different from those noted in the first fractionation.

Probable Formula.

Manifestly the empirical formula indicated for metahewettite nearly saturated with water at 35° is $CaO, 3V_2O_5, 9H_2O$. What it may have been when the minerals were originally deposited can not at present be said, although from the curve for water content under varying hygrometric conditions at summer temperatures (Fig. 2), which shows only very slight changes when the humidity is high, it might be argued that the limit of hydration has been reached with nine molecules of water. The effect of low temperatures is,

however, unknown, and it may be that the original water content was higher.

The same statements apply to hewettite, but here the excess of 0.6 molecule of water above nine molecules seems to be real, since it is by no means accounted for by the fraction of one per cent. represented by the nearly horizontal upper part of the curve, which fraction might be considered hygroscopic or absorbed water.[16] It is conceivable that this excess is connected with molybdenum, but much more likely that it has to do with a vanadyl-vanadic compound as already suggested (p. 41).

For the moment we will assume that the formula of both minerals, when holding the maximum amount of water, is $CaO, 3V_2O_5, 9H_2O$. Of what acid, then, are they salts?

The ratio of CaO to V_2O_5 shows that they cannot be salts of orthovanadic acid. Moreover, the known orthovanadates are very few in number and exhibit little stability, but pass readily into hexavanadates. From the fact that six of the nine molecules of water are quickly removable at ordinary temperatures in dry air and the others are much more firmly held, it might seem justifiable to assume six molecules of water of crystallization and three of constitution. Such disposition of them necessitates derivation of the minerals, as quarter-saturated salts, from the hypothetical acid $H_8V_6O_{19}$, an octobasic hexavanadic acid, a possible derivative of orthovanadic acid. We are confronted, however, with the fact that neither such an acid nor salts of it are known. Hexavanadates, however, derived from the tetrabasic acid $H_4V_6O_{17}$, also a derivative of orthovanadic acid, have been described and they resemble in general the two minerals in question, so far as can be determined from the meager data available.

Tetrabasic hexavanadic acid offers the possibility of two isomers of a salt of a bivalent metal. It seems then necessary to consider these minerals as acid salts of this acid, and the name metahewettite for one of them is not only justified but appropriate. If this reference is proper, only one molecule of water of constitution is possible

[16] Not all of the hygroscopic or absorbed water was necessarily removed in the first part of the dehydration over sulphuric acid.

and the other eight are water of crystallization. Against two of the eight being regarded as water of crystallization might be advanced the difficulty of removing the seventh and eighth molecules, but this can not be considered a weighty argument. Neither can the fact that the water content is so markedly affected by outside humidity and is susceptible of repeated removal and restoration be brought as a conclusive argument against the assumption of water of crystallization, for it must be remembered that loss of water is not accompanied by rupture of the crystalline structure, as is usually the case with true hydrates. On the other hand, in favor of water of crystallization, as opposed to water of absorption, must be placed the breaks in the curves of dehydration observed when the minerals are rapidly heated above 100°, and the fact that the content of water at a fixed atmospheric temperature does not bear a continuous relation to the outside humidity.

On the whole we are disposed to adopt the view that eight of the nine molecules represent water of crystallization and to report the formulas of both minerals as examined by us to be $CaH_2V_6O_{17}\cdot 8H_2O$. Under natural conditions mixtures of this and another salt of much lower hydration may and do often occur.

Inspection of the formulas of the artificial alkali and alkaline-earth hexavanadates throws no light on this problem, since the range in number of molecules of water is very wide for the normal alkali salts and the number is reported as 14 for the normal barium salt and 9.5 for the normal magnesium salt. Acid salts of bivalent metals do not seem to have been prepared, unless Ditte's calcium "trivanadate" $CaO,3V_2O_5,12H_2O$, is such a salt, perhaps identical with one of the minerals described by us. Its description, however, does not fit our minerals, since it is reported as very soluble in water and as having the luster of gold. The evidence of the salts mentioned points, however, to the possibility that hewettite and metahewettite may have held more than nine molecules of water when formed, unless the fact that only slight changes occur in their water content with high atmospheric humidity at summer temperatures negatives such a possibility.

Under the name alaite K. A. Nenadkevich has given a very brief

description[17] of a dark red, silky, soft and dense, moss-like mineral to which he assigned the formula V_2O_5,H_2O. The description fits hewettite very well in the main and it will be of interest to learn if on further study alaite may not prove to be a calcium vanadate related to, if not identical with, hewettite or metahewettite. Alaite is one of a number of vanadium and uranium minerals occurring in the Province of Ferghana, Russian Turkestan.

Pascoite.

Not observed in the surface deposit at Minasragra, Peru, but formed on the walls of an exploratory tunnel since its excavation, is a mineral representing a further stage of neutralization by calcium than is shown in hewettite. The specimens examined were among those brought from Peru by Mr. Hewett and the analysis given below was made several years ago in the laboratory of the U. S. Geological Survey. The name proposed, pascoite, is from Pasco, the province in which the locality of occurrence lies.

Crystallographically, this mineral is unsatisfactory, since it occurs only in minute grains and clusters of grains, arranged in a way indicative of a crustaceous deposit—as though they were secondary in origin and had been precipated as a crust about preëxisting masses. No well-developed crystals suitable for goniometric measurement were observed, and only here and there in the crystalline aggregates were minute crystal faces seen. No distinct cleavage was noted, although occasionally indications of an imperfect pinacoidal cleavage were observed in grains under the microscope. The fracture is conchoidal. In color this mineral ranges from dark red-orange to yellow-orange; the more homogeneous masses being uniformly red-orange throughout. In thin flakes it is translucent and the clearer individuals are vitreous to subadamantine in luster, especially on flat crystal faces which glisten here and there in the aggregate. The streak is cadmium-yellow and the hardness about 2.5. The specific gravity is about 2.457, determined in methylene iodide and benezene, on the clearest and most homogeneous material. This value may be slightly low because a crystalline aggregate, instead of a single crystal, was

[17] *Bull. Acad. Sci. St. Petersburg*, p. 185, 1909.

PROC. AMER. PHIL. SOC., LIII,, 213, D, PRINTED JUNE 18, 1914.

used, and minute air spaces therefore may have been present between the grains.

In the powder form the grains are usually irregular in shape and colored more or less intensely in shades of orange, red, and yellow. Pleochroism is noticeable; α = light cadmium-yellow; β = cadmium yellow; γ = orange. Absorption $\gamma > \beta > \alpha$. The refractive indices were determined by the immersion method; mixtures of methylene iodide, arsenic tribromide and arsenic sulphide (realgar) being used for the purpose. Owing to the color of the mineral, however, the phenomena on which the refractive index determinations by this method rest were less sharply marked than usual, and the probable error is correspondingly larger.

$$\alpha = 1.775 \pm .005$$
$$\beta = 1.815 \pm .005$$
$$\gamma = 1.825 \pm .005$$

The birefringence is strong and gives rise to high interference colors, even in comparatively thin grains. By direct determination, $\gamma - \alpha$ was found roughly to be about .050. The optic axial angle was measured by the double screw micrometer ocular[18] on sections showing only one axial bar and also on a section normal to the acute bisectrix. For sodium light 2V was found to be $50°.5 \pm 1°$ or 2E is about 100°; for lithium red light 2V is about $56° \pm 3°$, or 2E, about 115°. The determination in lithium light was much less satisfactory and accurate than that in sodium light. The dispersion of the optic axes is very considerable with $2V_{Li} > 2V_{Na}$ and its effect is clearly marked in the interference figure. The appearance of the interference figure shows, moreover, remarkably strong crossed dispersion—so strong in fact that in white light a section normal to the acute bisectrix never extinguishes completely, but near the position of extinction for light of any wave-length shows abnormal interference colors in characteristic tones, especially of green and orange. On a section nearly normal to the acute bisectrix the position of total extinction for sodium light made an angle of about 8° with that for lithium light. This angle, $\gamma_{Li} : \gamma_{Na} = 8°$, is only ap-

[18] *Am. J. Sci., 24,* 317–369, 1907.

proximatetly correct, and may be several degrees in error, owing to the weakness of the lithium light source used, and consequent lack of sharpness of position of total extinction.

The above optical data indicate that this mineral is in all probability monoclinic in crystal system, with its optic axial plane normal to the plane of symmetry. To summarize, the determinative optical characteristics of this mineral are: Crystal system, probably monoclinic; axial ratio, unknown; cleavage, poor and probably after 010. H, about 2.5; sp. g., about 2.46. Color, dark red-orange to yellow-orange; luster, vitreous to sub-adamantine. Pleochroism, noticeable, γ = orange; β = cadmium-yellow; α = light cadmium-yellow. Absorption, $\gamma > \beta > \alpha$. Refractive indices, $\alpha = 1.775 \pm .005$; $\beta = 1.815 \pm .005$; $\gamma = 1.825 \pm .005$.

Birefringence is strong. $2V_{Na} = 50°.5 \pm 1$. $2E_{Na}$ about 100°. $2V_{Li} = 56° \pm 3°.$; $2E_{Li}$ about 115°. Dispersion, crossed and strong, Optical character —. Plane of optic axes normal to plane of symmetry.

On the whole, the material is homogeneous and comparatively free from inclusions and suitable for chemical work. Here and there foreign material was observed, but in the material selected for chemical analysis it was not present in sufficient quantity to veil seriously the chemical relations.

Pascoite melts readily, forming a deep red liquid, and is easily soluble in water.

ANALYSIS OF PASCOITE.

	Per Cent.	Mol. Ratio.
V_2O_5	64.6	3.18
MoO_3	.3	——
CaO	12.6	2.00
H_2O 100° —	13.8	6.87
H_2O 100° +	7.8	3.88
Undet. and loss	.9	——
	100.0	

The ratios are not as satisfactory as could be desired. The values approach those required from the formula $Ca_2V_6O_{17} \cdot 11H_2O$, which calls for: V_2O_5, 63.76; CaO, 13.10; H_2O, 23.14. Very recent tests show that almost no loss of water occurs at room temperatures until

the surrounding humidity is reduced practically to zero. When dehydrated over P_2O_5 the color is no longer orange but dirty yellow. After rehydration in moist air the color is much brighter yellow, but without any trace of the original orange.

As with hewettite and metahewettite, the amount of water evolved at and below 100° is almost exactly removable by exposure over strong sulphuric acid at room temperature for one or two months and much more rapidly in a vacuum. Above 100° further loss begins, but is complete only at a temperature of perhaps 300°. No experiments have been made as yet to trace the progress of dehydration at temperatures above 100°. Further tests on this mineral are needed and will be made if opportunity offers.

The arguments advanced (pp. 46–48) for hewettite and metahewettite with respect to their chemical classification apply to pascoite also. In this case, if the assumptions made for the former minerals are justified, we have normal calcium hexavanadate with x molecules of water, at least 11 when the mineral was formed.

Analytical Procedure.

The methods of analysis need no special mention except as to the separation and determination of the vanadium and molybdenum. As a rule the portions used for water determinations served also for the other constituents. They were treated in a glass tube, with dry hydrochloric acid gas, after solution in nitric acid and evaporation to dryness in a porcelain boat on a hot plate. The brown vapors were collected in receptacles containing a little water. Two of these in series were sufficient, but a third was sometimes used, all so connected that no back suction of liquid was possible if the gas stream slackened. The material in the boat is attacked instantly the acid vapor reaches it, even without the aid of heat, but the reaction is not complete in one operation even when heat is applied after vigorous action ceases. It is necessary, usually, to remove the boat, to reconvert the contents to nitrates and to repeat the treatment with hydrochloric acid gas several times, and to wash out and dry the glass tube between each operation.

The molybdenum, less volatile than the vanadium, comes off only,

or at least for the most part, during the later periods and upon heating. If present in some quantity it reveals itself by a white crystalline deposit in front of the boat.

The contents of the receptacles and of the tube were finally evaporated in porcelain with sulphuric acid, which was then heated till fumes arose. After dilution, the deep blue solution was transferred to a flask, saturated with hydrogen sulphide gas, and heated while the gas still passed. The flask was then stoppered and allowed to stand, over night as a rule, before filtering. The molybdenum sulphide was roasted to oxide.

The filtrate was brought to boiling in a flask while passing carbon-dioxide gas until hydrogen sulphide was wholly expelled, then titrated in the flask at 70°–80° with permanganate. The vanadium was again reduced, this time with sulphur-dioxide gas, which in turn was expelled by boiling in a current of carbon dioxide, and the vanadium was again titrated. If desired the operations of reduction and titration were repeated. The values obtained after successive repetitions of the reduction by sulphur dioxide agreed well but were always somewhat lower than after reduction by hydrogen sulphide. The difference is no doubt due to the presence in the one case of a little free sulphur from the hydrogen sulphide, which consumes permanganate at the high temperature of titration. If the molybdenum sulphide has been filtered through paper instead of a Gooch crucible, permanganate is also consumed by organic extracts from the paper.

Summary.

Two apparently different calcium vanadates are described, which resemble each other very closely and have the same composition—$CaO \cdot 3V_2O_5 \cdot 9H_2O$—when holding their maximum water content at room temperatures. One of them—hewettite—occurs at Minasragra, Peru, and has been noticed on a single specimen from Paradox Valley, Colorado. The other—metahewettite—occurs at numerous localities in western Colorado and eastern Utah. Both minerals are sparingly soluble in water.

A third calcium vanadate—pascoite ($2CaO \cdot 3V_2O_5 \cdot 11?H_2O$)—is also described. This occurs with hewettite at Minasragra. It is very soluble in water.

The first and second minerals are regarded as hydrated acid hexavanadates—$CaH_2V_6O_{17}\cdot 8H_2O$—the third as a normal hexavanadate, $Ca_2V_6O_{17}\cdot 11\,?H_2O$.

The reasons for specific separation of hewettite and metahewettite are set forth in detail. The two minerals are so sensitive to changes in atmospheric humidity that their water content varies within wide limits at different times of the year. The removal of all or nearly all the water does not result in breaking down of the crystal structure, and until this has occurred the water is wholly or in great part taken up again when opportunity is offered.

The importance is emphasized of bringing all minerals that behave in this way to a definite maximum water content before analyzing them and of following carefully the course of dehydration under prescribed conditions. Detailed directions are given for such tests and for avoiding several sources of error.

Attention is also called to two fairly constant associates of metahewettite. One of these (also a constituent of carnotite ores) is a gray hydrous silicate of aluminum, trivalent vanadium, and potassium. The other is elemental selenium, the existence of which as a mineral species seems now for the first time established.

ACKNOWLEDGMENTS.

Our thanks are due primarily to Mr. D. Foster Hewett for the material from Peru and to Messrs. T. V. F. Curran, R. H. McMillen and A. G. McNaughton, also to Mr. Frank L. Hess and Dr. W. T. Schaller of the U. S. Geological Survey, for North American material and for valuable information. Dr. Wm. Blum and Mr. E. C. McKelvy, of the Bureau of Standards, were of material assistance in arranging the thermostat and electric furnace used for the water determinations.

BUREAU OF STANDARDS AND GEOPHYSICAL
LABORATORY OF THE CARNEGIE INSTITUTION,
WASHINGTON, D. C., April, 1914.

THE INFLUENCE OF ATMOSPHERIC PRESSURE UPON THE FORCED THERMAL CONVECTION FROM SMALL ELECTRICALLY HEATED PLATINUM WIRES.

By A. E. KENNELLY and H. S. SANBORN.

(*Read April 24, 1914.*)

Object of Enquiry.

This paper describes the process and results of a research made at Harvard University in 1911, to determine the effect of change in atmospheric pressure on forced thermal convection from thin platinum wires. By forced thermal convection is meant the carrying away of heat from the surface of a wire by wind-motion, *i. e.*, by a rapid transverse movement of the wire through the surrounding air. This wind motion through the air dissipates the heat from the wire convectively. The rate of thermal convection depends upon the length and diameter of the wire, its surface condition, the temperature elevation of the wire above the air, the velocity of the motion, and the pressure of the air. The object of the enquiry was to determine the effect of the last-named variable—variation of atmospheric pressure—upon the thermal dissipation, the other quantities being kept constant.

History of the Enquiry.

The research here described was the outcome of an earlier investigation on "The Convection of Heat from Small Copper Wires," by Messrs. A. E. Kennelly, C. A. Wright and J. S. Van Bylevelt, presented at the Frontenac Convention of the American Institute of Electrical Engineers, June 28, 1909, and published at p. 363, Vol. XXVIII., part I., of the *Transactions* for that year. In that research, the forced convection of heat from a thin copper wire, electrically heated to a constant temperature, *i. e.*, maintained at a constant

electric resistance, was discovered to vary as the square root of the wind velocity, which was measured by the speed of the moving wire through otherwise tranquil air. In other words, it was discovered experimentally that in order to dissipate double the power from the wire, at constant resistance and temperature-elevation, it was necessary to quadruple the speed of the wire through the air. This relation was found to hold, within observation errors, for several different sizes of thin copper wire, and for various temperature elevations, between wind-velocities of 2 and 20 meters per second. Below 2 meters per second, the relation deviated towards the case of free convection from a hot wire at rest. That is, at low wind velocities, empirical corrections became necessary for the free convection which naturally occurs from a wire at rest, or moving at zero speed through the air. The possible application of the square-root law of wind cooling to anemometry was also pointed out.

After the results were published in 1909, our attention was drawn to papers by Professor Boussinesq in the *Comptes Rendus* for 1901, Vol. 133, p. 257, and the *Journal de Mathématiques,* 6th Series, Vol. 1, 1905, in which is given the theory of the convection of heat by a stream of liquid from the surface of a cylindrical rod, placed at right angles to the stream. The liquid is assumed to be incompressible and devoid of viscosity. The formula arrived at by Boussinesq, as given by Russell, is:

$$H = 8\theta \sqrt{\frac{s\sigma k V a}{\pi}},$$

where H is the heat carried off convectively per second from unit length of cylinder.

s is the specific heat of the liquid.
σ is the density of the liquid.
k the thermal conductivity of the liquid.
V is the velocity of the liquid.
a the radius of the cylinder.
θ the temperature elevation of the cylinder.

This means that the linear forced convection, or ergs per second per cm. of the cylinder, is proportional to its temperature-elevation above

the liquid, and to the square root of the specific heat, the thermal conductivity, the wind velocity, the fluid density and the wire radius.

Dr. Alexander Russell communicated an important paper on the theory of the subject to the Physical Society of London in July 1910,[1] developing and extending Boussinesq's formula.

Professor J. T. Morris has recently successfully applied the square-root law of forced-convection velocity to the measurement of wind-velocities, using an ingenious form of Wheatstone bridge for this purpose. His observations were communicated to Section G of the British Association in 1912[2] and also to "Engineering"[3] in 1913. His results have confirmed the application of the law for wires of various metals up to diameters of 0.3 mm.

The papers and deductions of Boussinesq were not known to us at the time we presented our former paper in 1909; but since the square-root law of velocity arrived at theoretically by Boussinesq in 1901–1905, for an incompressible non-viscous liquid, has been found to hold within errors of observation for ordinary air, it became desirable to ascertain whether the linear forced convection of air varied as the square root of the air pressure, as suggested by Boussinesq's formula.

Method of Measurement Employed.

The method followed and the apparatus used were respectively the same as those described in the A. I. E. E. paper of 1909, above referred to. A short length of the thin wire to be tested was held in a fork, and was driven by an electric motor at successively varied speeds in a large steel tank, the atmospheric pressure within which was kept constant in each series of tests; but was different in different series.

Test Wire.

The wire used in all of the tests here described was of good commercial platinum, No. 36 B. & S. gauge, with a mean diameter of

[1] *Proc. Physical Society*, 1910, Vol. XXII., also *Phil. Mag.*, October, 1910.

[2] Prof. J. T. Morris, "The Electrical Measurement of Wind Velocity," *The Electrician*, Oct. 4, 1912, pp. 1056–1059.

[3] J. T. Morris, "Distribution of Wind Velocity about a Circular Rod," *Engineering*, Vol. 96, pp. 178–181, Aug. 8, 1913.

0.114 mm. (0.0045 inch). In the tests of 1909, copper wires were used. The advantage of copper is that its resistivity temperature-coefficient is relatively large, and is fairly reliable. On the other hand, hot copper wires oxidize superficially when driven through the air, and are therefore subject to variation in convective dissipation, owing to this change of surface condition. As the test-wire in the new measurements had to be driven inside a steel tank, with only occasional inspections, it was decided to employ platinum, instead of copper; although the resistivity temperature-coefficient of the platinum was but little more than half that of copper; so that the resistance of such a platinum wire is not so sensitive to changes of temperature as a copper wire. Consequently, greater care was needed in the electrical measurements of resistance in the platinum test-wire, in order to determine the temperature elevation.

A measurement of the temperature-coefficient of resistivity of the platinum wire used was made by immersing 5.5 meters of it on a reel in an oil-bath, and measuring the resistance at twelve different temperatures between 0° C. and 100° C. As shown in Fig. 6, the results obtained lie close to the straight line:

$$\rho_t = \rho_0(1 + 0.002575t) \qquad \text{absohm-cm} \quad (1)$$

where ρ_t is the resistivity at $t°$ C. (absohm-cm.), and ρ_0 is the resistivity at 0° C. (absohm-cm.).

The particulars concerning the test wire are given in the accompanying Table:

TABLE I.

TEST-WIRE DIMENSIONS AND DATA.

Mean Diameter.		Cross-Sectional Area, sq. cm.	Linear Surface, sq. cm./cm.	Linear Mass, gm./cm.	Linear Res. at 0° C., absohms/cm.	Resistivity at 0° C., absohm-cm.	Temp. Coeff. of Resistivity 0° C.
mm.	Inch.						
0.114	0.0045	1.02×10^{-4}	0.0358	2.415×10^{-3}	1.28×10^{8}	1.306×10^{4}	0.002575

TEST-WIRE HOLDER.

The test-wire was held in a fork or frame, mounted on the shaft of the driving motor. The fork is indicated in Fig. 1. It is counterpoised by the sliding weight *f*. The test-wire is shown at *b*, held

straight and fairly tight, by the elasticity of the brass strip prongs *aa'*, aided by the tension-screws gg^1. Current is steadily supplied to the test-wire through slip-rings *cc'*, on which rest stationary copper gauze brushes of square cross-section 0.64 cm. (¼ inch) on each

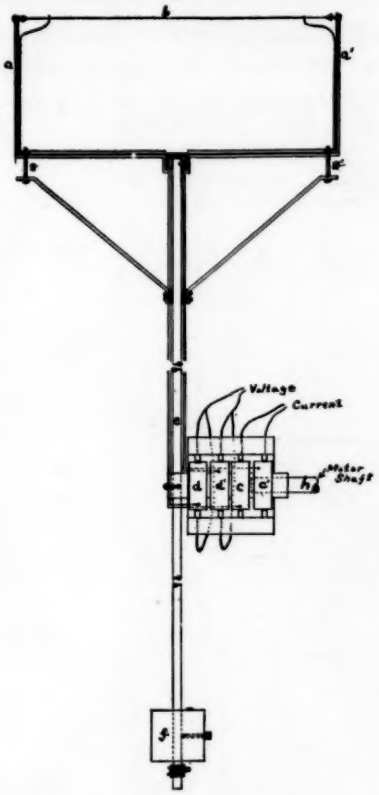


FIG. 1. Details of Rotatable Fork Supporting the Test Wire.

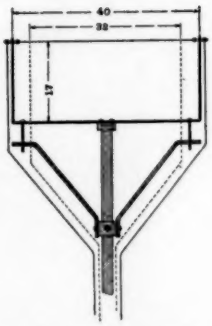


FIG. 2. Details of Fork. Dimensions in Centimeters.

edge. The details of the fork dimensions are shown in Fig. 2. At two points along the test-wire 32 cm. apart, pressure wires are soldered to the test-wire. These pressure wires are of platinum, of the same size as the test-wire. They connect with insulated copper wires fastened to the sides of the fork, and terminate in the slip-rings *dd'* carried by the motor shaft, on which rest two pairs of

stationary gauze brushes. The electrical connections are as shown in Fig. 3. The source of e.m.f. was a storage battery. The regulating resistance *RR* was so adjusted that the ratio of the p.d. between pressure-wires, to the current strength, was equal to a predetermined resistance. That is, the current in the test-wire was gradually increased until the ratio of the reading of the voltmeter *V* to that of the ammeter *A*, was found, by slide-rule, to give the correct resistance sought to be maintained in the test-wire at all wind speeds.

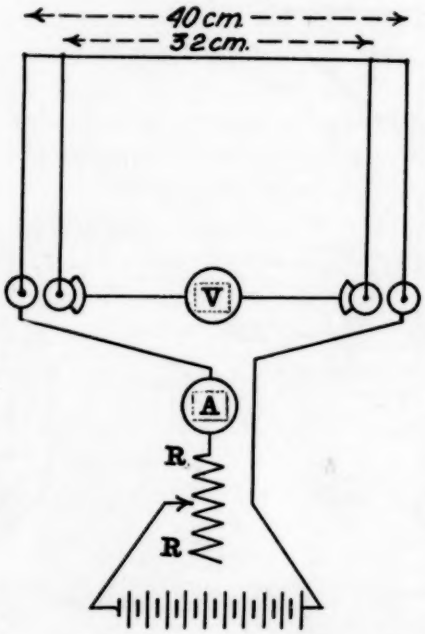


FIG. 3. Diagram of Electrical Connections.

As the driving speed increased, the current supplied to the wire had to be increased, in order to maintain this ratio V/A. When, on the contrary, the driving motor was brought to rest, the current in the test-wire had to be reduced to a relatively small value, in order to reproduce the ratio.

The fork was mounted on the shaft of a ½-HP. 115-volt direct-current shunt motor, arranged to run at adjustable speeds. The

wind speed of the test wire in cm. per sec. was taken as $2\pi \times$ cm. fork radius $\times$ speed of motor in r.p.s. At the other end of the motor-shaft was coupled a small magneto-generator for indicating, by its e.m.f., the speed of rotation. The fork-motor-magneto mechanism is illustrated in Fig. 4, supported on a wooden frame intended to be held in place inside the pressure tank, which is shown with the manhole open.

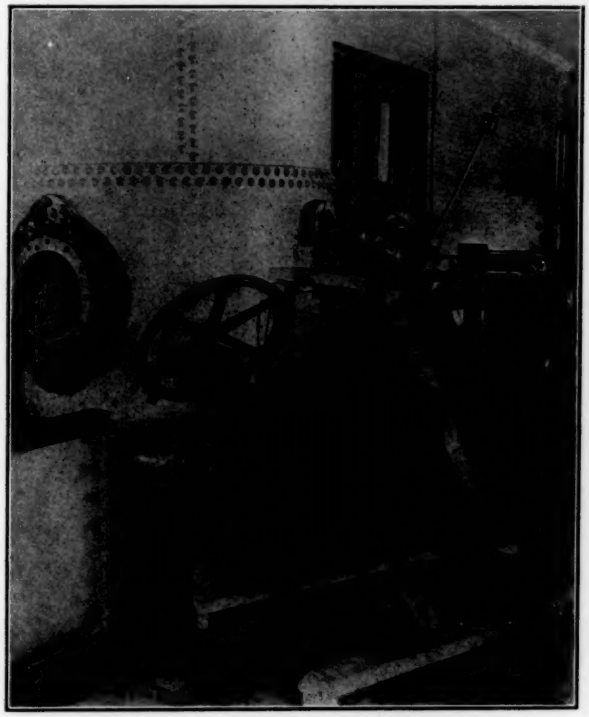


FIG. 4. Photograph of Fork, Driving-Motor, Magneto Speed-Indicator and Pressure Tank.

PRESSURE TANK.

The pressure tank in which the motor and fork were supported was a vertical steel cylinder of ½″ (1.25 cm.) steel plates, riveted. Figure 5 shows the dimensions of the tank, and also the position within it occupied by the motor and fork. The radius of the fork

to the test-wire; *i. e.*, the distance of the test-wire from the motor-shaft axis was 58.5 cm. (23″); and the radius of the pressure tank was 76 cm. (30″), leaving a clearance between the rotating wire and the tank wall of 17.5 cm. (7″). A larger pressure tank, allowing

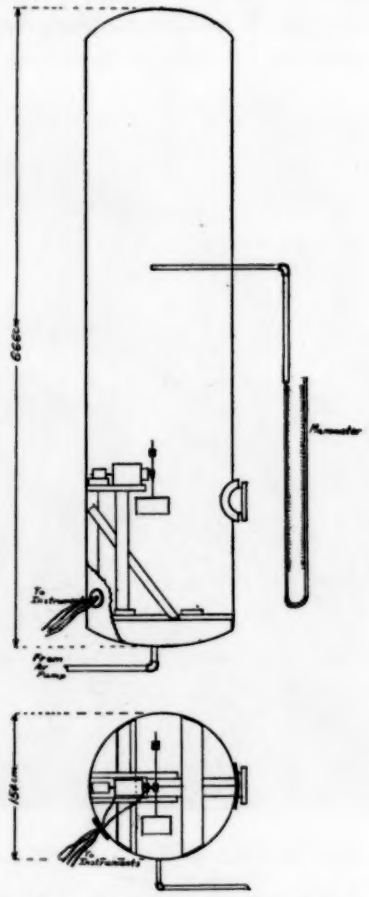


FIG. 5. Elevation and Plan of Pressure Tank showing position occupied by the Test-Wire.

more space and clearance for the revolving test-wire, would have been preferable; but the arrangement was the best that could be made with the apparatus at hand. The results obtained at any

single air-pressure were not so good as those obtained at normal atmospheric pressure in the open air outside the tank, with a larger fork radius, and free air-space. That is, the curves of linear convection against wind-velocity, on logarithm paper, showed more tendency to deviate from a straight line, in these tank tests than in open-room tests, both at low speeds and at high speeds. These deviations might perhaps be explained by air-churnings in the tank, due to the motion of the fork and wire in a somewhat confined space.

The insulated wires leading to motor, magneto, and test-wire, were brought out through holes in a wooden plug bolted air-tight over a manhole.

The speed of rotation of the motor inside the tank was measured in two independent ways; namely (1) by the e.m.f. of the little magneto-generator coupled to the motor, (2) by a contact made

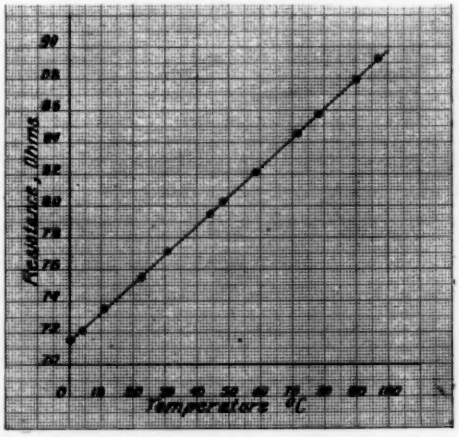


FIG. 6. Resistance in oil of 550 cm. of the Platinum Test Wire at different temperatures between 0° C. and 97° C. in order to determine the resistivity temperature-coefficient.

through a wire on the motor shaft once in each revolution, which gave a click in a telephone. The speed calibration of the magneto and its voltmeter could thus be checked, from time to time, by counting the telephonic clicks in one minute.

The pressure of the air in the tank was controlled by pumps connected with the tank. The tank was fairly tight and ordinarily held its pressure steadily during a test. A large glass U-tube containing mercury was connected with the tank. The difference of level between the mercury in the two arms of the U, corrected for temperature, gave the difference of pressure between the air inside and outside the tank. The absolute pressure of the air in the tank was thus the sum of the U-tube pressure and the corrected barometer pressure outside. This absolute pressure was expressed in "bars" or C.G.S. units (dynes per sq. cm.), by allowing 75.009 cm. of mercury to 1 megabar or 10^6 bars.[4]

Hot-Wire Temperatures.

Two hot resistances were selected for the 32 cm. length of test-wire in different series of tests; namely one at 8.44 ohms, and the other at 10.0 ohms, corresponding to temperatures of 410° C. and 558° C. respectively, by extrapolation from the calibration test between 0° C. and 100° C. indicated in Fig. 6. These temperatures are therefore inferred by resistance. If the temperatures of the wire actually differed from the above inferred values, the values of linear convection here deduced would be correspondingly changed; but the comparative results would be unchanged. So far as the main subjects of enquiry are concerned, it is sufficient that the wire returns to one and the same definite temperature when heated electrically to one and the same resistance. With the air-temperature in the tank in the neighborhood of 20° C., the inferred temperature-elevation of the test-wire by resistance was 390° C. and 538° C. About ten series of speed-measurements were made at each of these elevations, with different air-pressures.

The following table gives one series of tests as an example.

[4] "Les Récents Progrès du Système Métrique," Paris, Gauthier-Villars, 1907, pp. 30–31.

TABLE II.

SERIES OF MEASUREMENTS ON FEBRUARY 16, 1911. Observers A. E. K. and H. S. S. Pressure in tank 75.2 cm. Hg above that in room. Barometer 778 mm. at 14.5° C. Temp. air in tank 18.5° C. Mean absolute pressure in tank 2.04 megabars. Res. of test platinum wire between pressure wires kept at 8.44 ohms. Inferred temp. elevation 391.5° C. R. P. M. of driving motor = 1.14 × magneto voltage.

I. Magneto Volts.	II. Wind Velocity v cm./sec.	III. P. D. on 32 cm. E abvolts.	IV. Current in Wire I abs. amp.	V. Linear Dissipation $P_e = E.I/32$ abwatts/cm.	VI. Linear Dissipation per ° C. P_e/θ abwatts / cm. deg. C.	VII. $\frac{P_e}{\theta\sqrt{v}}$.	VIII. $\frac{P_e}{\theta\sqrt{v+30}}$.
		$\times 10^8$		$\times 10^7$	$\times 10^4$		
101	705.5	23.5	0.279	2.051	5.24	1973	1930
126	880.5	24.9	0.295	2.294	5.86	1975	1942
149	1041	26.0	0.308	2.501	6.390	1981	1952
174	1216	26.95	0.320	2.695	6.880	1975	1948
203	1418	27.8	0.330	2.873	7.340	1949	1928
225	1572	28.55	0.339	3.030	7.740	1954	1929
250	1747	29.3	0.348	3.194	8.160	1954	1935
274	1914	30.0	0.356	3.343	8.539	1952	1935
303	2117	30.6	0.363	3.476	8.879	1930	1915
310	2166	30.9	0.367	3.553	9.075	1950	1938
100	699	23.35	0.277	2.023	5.167	1954	1912
74	517	21.75	0.258	1.754	4.480	1970	1916
43	300.4	19.55	0.232	1.420	3.628	2092	1995
0	0	10.5	0.125	0.412	1.050	∞	1917

Column I gives the voltage generated by the magneto on the driving-motor shaft. From these readings, the speed of the test-wire through the air in the tank; or the "wind-velocity" v of Column II is directly derived in cm. per sec. Column III gives, at each steady speed, the P.D. between pressure wires in C.G.S. magnetic absolute units or "abvolts." Column IV gives the corresponding current strength in C.G.S. magnetic absolute units or "absamperes." The ratio E/I is approximately constant at 8.44×10^9 absohms, or 8.44 ohms. Column V gives the linear dissipation of heat from the wire, or the abwatts (ergs per second) per cm. of wire length. Column VI gives this linear dissipation per deg. Cent. of inferred temperature elevation. It will be seen that this varies from 1.05×10^4 abwatts per cm. and ° C. at standstill, up to 9.075×10^4 at 2,166 cm. per sec. Column VII gives the entries of VI divided by $\sqrt{v}$ giving a value nearly uniform about 1,960, until the velocity v falls to 300 cm. per sec. At standstill, of course, owing

to free convection, the value becomes infinite. If, however, we add 30 cm. per sec. to all the wind velocities v, to correct empirically for free convection as described in the paper of 1909, we obtain the values given in the last column VIII, which do not differ greatly from 1,930 abwatts per cm. ° C. and $\sqrt{v}$, at all speeds in the table.

It will be observed that no correction is made for loss of heat by radiation from the test-wire. That is, the linear dissipations in column VI are treated as though entirely due to convection. In our paper of 1909, a correction was attempted for radiation, on the basis

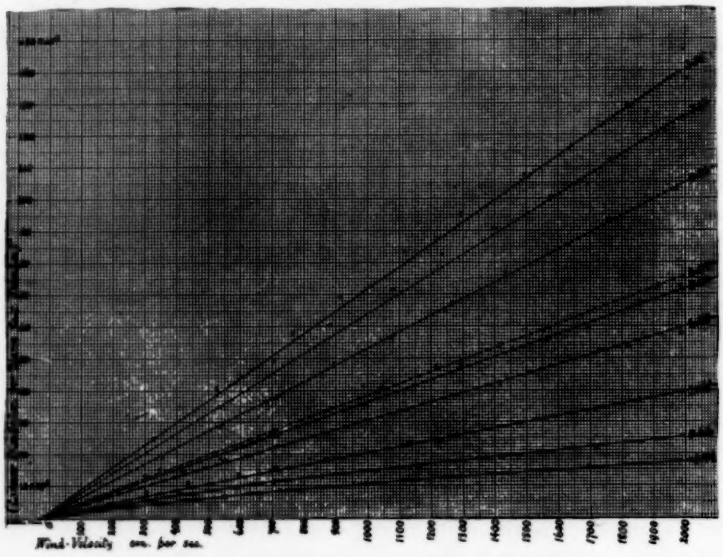


FIG. 7. Curves of $\left(\frac{P}{\theta}\right)^2$ against v, for $\theta = 390°$.

of Stefan's formula. Since, however, it has been pointed out by Dr. Langmuir[5] that the radiation from platinum according to Hagen and Ruben's formula is only a small fraction of that from a "black body," or perfectly non-reflecting radiator, at the same temperature, the radiation corrections in the case of Table II are nearly all less than 1 per cent. of the dissipation, and it has therefore been omitted throughout.

[5] "The Convection and Conduction of Heat in Gases," by Irving Langmuir, *Proc. Am. Inst. El. Engrs.*, June 25, 1912.

The relation found in our 1900 paper was that

$$P_c = k\theta\sqrt{v} \qquad \text{abwatts per cm.} \quad (2)$$

where P_c is the linear convection from the hot wire in abwatts per cm., θ the temperature elevation of the wire, in degrees Centigrade, v the wind-velocity or speed of transverse motion of the wire through the air in cm. per second, and k a constant depending, among other things, on the size and surface-condition of the wire. This formula was found to hold well between the wind velocities $v = 200$ and $v = 2000$ cm./sec. (7.2 and 72 km/hr. or 4.47 and 44.7 statute miles/hr.); but not to hold below $v = 200$, unless 30 cm. per sec. were added as an empirical correction to all speeds to take free convection into approximate account. This empirical correction, applying fairly well, gave:

$$P_c = k\theta\sqrt{v + v_0} \qquad \text{abwatts per cm.} \quad (2a)$$

where v_0 is a virtual velocity of free convection approximating 30 cm. per second.

The relation indicated in (2) can be presented graphically by straight lines on logarithm-paper, but Professor Morris has employed the corresponding relation:

$$\left(\frac{P_c}{\theta}\right)^2 = k^2 v \qquad \left(\frac{\text{abwatts per cm.}}{\text{deg. C.}}\right)^2 \quad (3)$$

That is he plots the square of the observed linear convection per degree C., against the wind velocity, thus producing a straight line, if either (2) or (2a) applies. The procedure is followed in Figs. 7 and 8. Thus, taking Fig. 7, the broken straight line marked 2.04 corresponds to the results in an air-pressure of 2.04 megabars, and the observations in Table I appear on or near this line as small circles. Nine different series are indicated in Fig. 7 at pressures of 0.44, 0.66, 1.02, 1.54, 2.00, 2.04, 2.81, 3.48 and 3.95 megabars respectively, the first two corresponding to vacua, 1.02 to normal atmospheric pressure, and the six others to extra pressure in the tank.

It will be seen that the two lowest curves—vacua—deviate distinctly from straight lines. The remainder are drawn as straight lines, and the observations conform to them fairly well, except at the

two highest pressures 3.48 and 3.95 megabars. This means that equations (2) and (3) held satisfactorily from 1 to 2.8 megabars, but did not hold so well outside those limits of pressure.

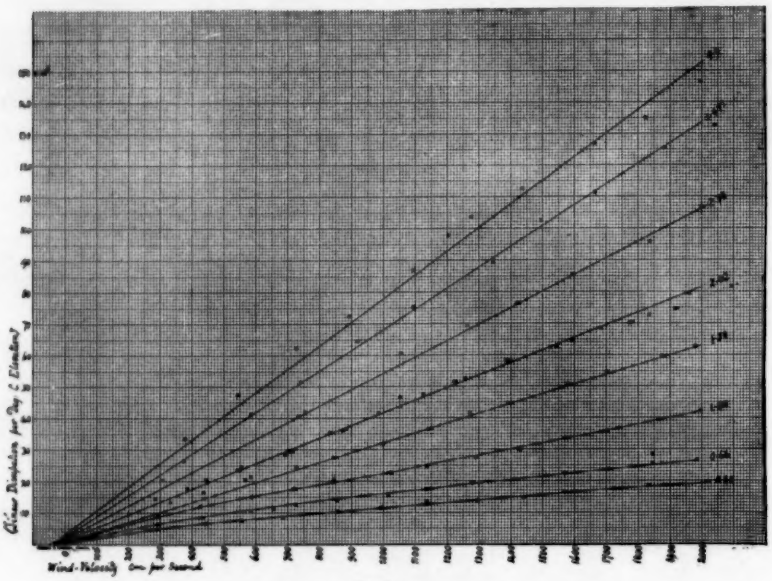


FIG. 8. Curves of $\left(\frac{P}{\theta}\right)^2$ against v, for $\theta = 538°$.

In order to eliminate, as far as possible, any disturbing influence on the forced convection in these tests due to the presence of water-vapor in the air contained in the tank, calcium chloride was kept in the tank. The measurements were all made between January 23 and February 17, 1911, at a time of the year when the air in Cambridge is ordinarily relatively free from moisture. In order to find whether moisture in the air had any considerable effect on the forced convection of heat from the test-wire, one test was repeated at one air-pressure (2 megabars) at each of the two temperature-elevations, first with the air after it had been exposed to the calcium chloride, and second with the calcium chloride removed and a dish of water set over night in its place. The actual difference in the humidity of the air in the tank was not measured, but it was supposed that there

would be a marked difference. It will be seen in Fig. 7 that the small circles on the 2.04 megabar line, representing the dried air test, fairly coincide with the small crosses representing the air test in the presence of water. The same is true for the 2.02 megabar line of Fig. 8. Consequently, the effect of moisture in the heat convection of moving air has not yet been determined from our tests, although it would seem reasonable that in view of the very appreciable known thermal capactiy of aqueous vapor, the effect of moisture might have been apparent.

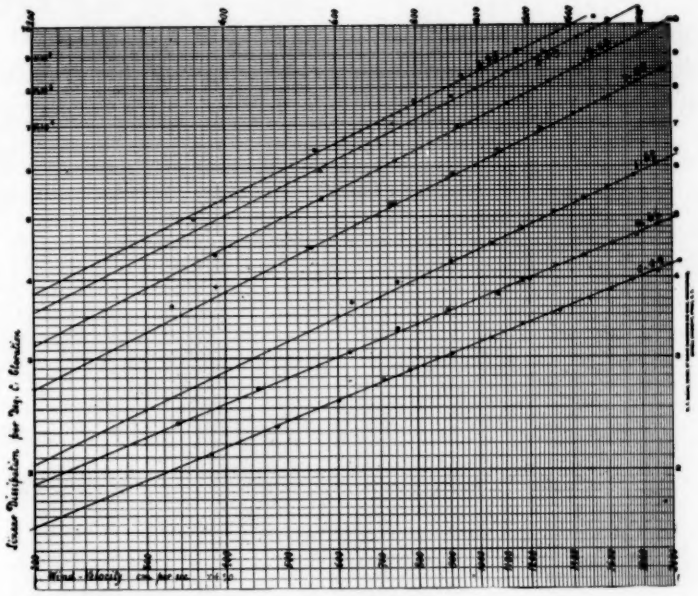


FIG. 9. Graphs of $\left(\frac{P}{\theta}\right)$ against v at different pressures for $\theta = 390°$ C. logarithm paper.

It will be seen that the observations in Figs. 7 and 8 indicate a relation:

$$\left(\frac{P_e}{\theta}\right)^2 = k^2(v + v_0) \qquad \left(\frac{\text{abwatts per cm.}}{\text{deg. C.}}\right)^2 \quad (4)$$

where v_0 is a velocity in the neighborhood of 30 cm. per second, which may be assumed as the empirical correction due to free convection

from the test-wire when held stationary in the air. Professor Morris's method of graphic representation has the advantage that it indicates directly the magnitude of the empirical correction v_0. If we take $v_0 = 30$, we have from (4)

$$\frac{P_e}{\theta} = k\sqrt{v + 30} \qquad \frac{\text{abwatts per cm.}}{^\circ\text{C.}} \qquad (5)$$

Figs. 9 and 10 give the graphs of P_e/θ on logarithm-paper, for the various sets of observations. It will be seen the observations lie not far from straight lines. These lines have a gradient of 1 : 2, or correspond to a square-root law, or exponent of 1/2 as in (5); except for the vacua (0.44 and 0.66 megabar), where the gradient is approximately 4 : 10; or would more nearly indicate a relation

$$\frac{P_e}{\theta} = k(v + 30)^{0.4} \qquad \frac{\text{abwatts per cm.}}{^\circ\text{C.}} \qquad (6)$$

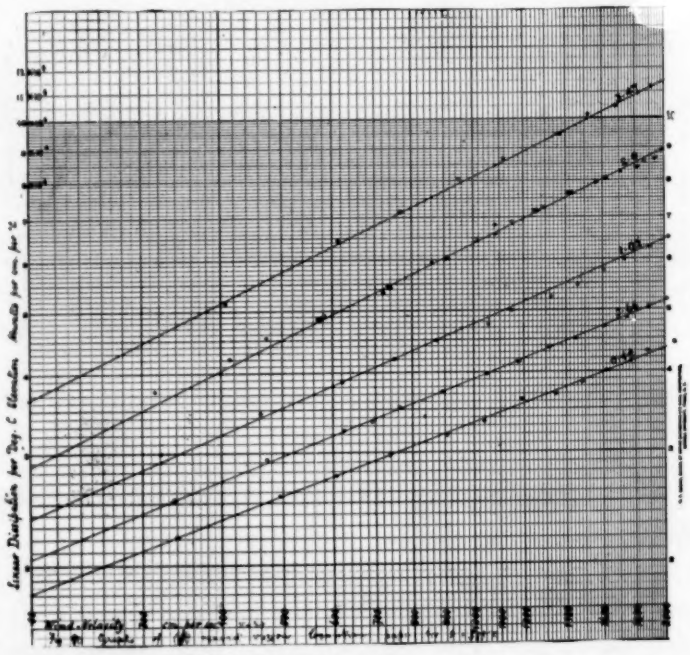


FIG. 10. Graphs of $\left(\frac{P}{\theta}\right)$ against $v + 30$ on logarithm paper for $\theta = 538^\circ$ C.

Considering the results indicated in Figs. 7 and 8, it appears that the slopes of the various straight lines are nearly proportional to the atmospheric pressure. This means that, at least to a first approximation:

$$\left(\frac{P_c}{\theta}\right)^2 = pk'(v + v_0) \qquad \left(\frac{\text{abwatts per cm.}}{\text{deg. C.}}\right)^2 \qquad (7)$$

where $pk' = k^2$

Consequently, when in Professor Morris's diagram, the atmospheric pressure p changes, the ordinates are increased in like proportion. We have then

$$P_c = \theta\sqrt{pk'(v + v_0)} \qquad \frac{\text{abwatts per cm.}}{\text{deg. C.}} \qquad (8)$$

so that the linear convection is nearly proportional not only to the square root of the velocity, but also to the square root of the atmospheric pressure. This agrees with Boussinesq's formula (o) when it is remembered that the air-density σ of the medium is proportional to the pressure p. The remaining constant k' involves, among other things, the diameter of the wire. According to Boussinesq's formula (o), the constant k' should be proportional to the square root of the wire diameter. A special investigation should be directed to this question: but the measurements recorded in our earlier paper of 1909 seem to indicate a higher ratio than the square root.

TABLE III.

VALUES OF k' IN THE EXPRESSION $P_c/\theta = \sqrt{k'p(v+v_0)}$ derived from the series of observations at different atmospheric pressures p, at the velocity $(v + v_0) = 1{,}000$ cm. per sec.

$\theta = 390°$.		$\theta = 538°$.	
p Bars.	k'	p Bars.	k'
3.95 x 10^6	1.87	4.0 x 10^6	1.98
3.48 "	1.87	3.47 "	1.96
2.81 "	1.85	2.79 "	1.90
2.04 "	1.83	2.02 "	2.00
2.00 "	1.76		
1.54 "	1.915	1.54 "	2.01
1.02 "	1.96	1.02 "	2.16
0.66 "	2.15	0.66 "	2.39
0.44 "	2.14	0.44 "	2.50

Table III shows the value of k' in formulas (7) and (8) for the various series of measurements appearing in this report at the velocity $v = 1{,}000$ cm. per second. It will be seen that k' varies between 1.76 and 2.50, with a mean value near 2.0.

Anemometer Measurements.

A wind-velocity measuring apparatus, or electric anemometer, was constructed of the same thin platinum wire as that used in the preceding tests (0.114 mm. diameter). A length of 25 cm. of this

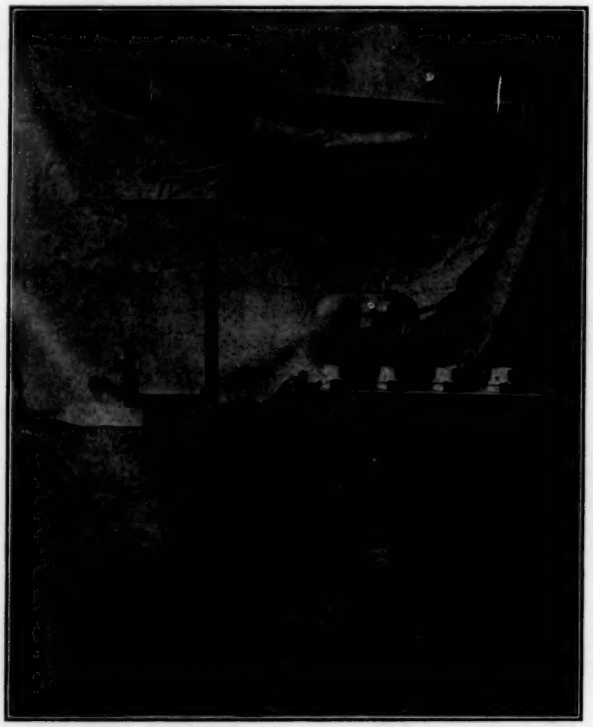


Fig. 11. Experimental Anemometer.

wire (10 in.) was supported vertically, between insulated clips, in a steel G frame shown in Fig. 11. Pressure taps, of the same size platinum wire, were soldered on to the vertical test-wire, at a distance

of 15 cm. (5.9 in.) apart. The vertical test-wire was then placed at the spot where the wind was to be measured, and heated by electric current. It thus served to measure horizontal wind-velocity in any direction.

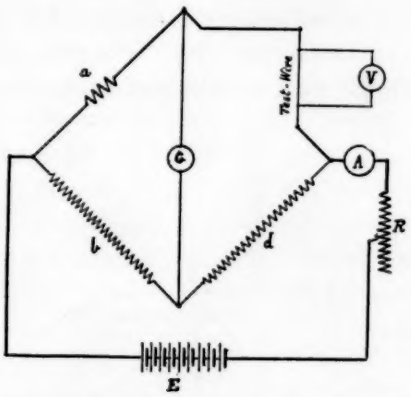


FIG. 12. Connections of Test-Wire for Indirect Measurements of Wind Velocity.

Two methods were used, one indirect-reading, the other direct-reading. In the indirect method, the connections were as shown in Fig. 12. Here the test-wire, shunted by a voltmeter, is placed in a Wheatstone bridge of unequal arms, so that the current in the test-wire side is ten times stronger than in the opposite side *bd*. The bridge is set for balance at a predetermined resistance and temperature of the test-wire. Whatever may be the horizontal velocity of the wind blowing over the test-wire, there is some current strength supplied to the bridge through a meter A, which will restore balance and zero current in the galvanometer G. When this balance is obtained, the readings of the ammeter A, and voltmeter V, are noted. Their product VA; or the voltage square V^2, is proportional to the power dissipated in the 15 cm. of test-wire, from which the velocity of the wind can be deduced with the aid of suitably prepared tables or curves. The advantage of this method is its relatively high precision. Its disadvantages are that it requires to be adjusted for each observation, and in gusty winds, it is not possible to secure a Wheatstone-bridge balance long enough to obtain readings of either V or A.

In the direct-reading method, the connections are as shown in Fig. 13. Here the test-wire is connected across 110-volt lighting mains, through an adjusted resistance, which may consist of incandescent lamps, so as to receive as nearly a constant heating current as is practicable. The voltmeter V is connected to the potential taps, 15 cm. apart on the vertical test-wire. The apparatus is then set up at the place where the wind is to be measured. The four leads are

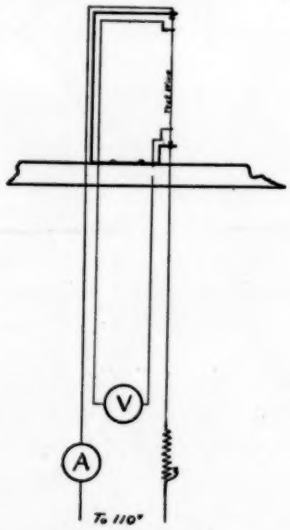


FIG. 13. Connections for Direct-Reading Type of Anemometer.

of any convenient length, and are bound up into a weatherproof cable. From calibration measurements made on a sample of the test-wire in a motor-driven fork, the linear convective dissipation of heat for any safe given linear resistance of the wire is known. As the horizontal component of wind-velocity increases, the temperature of the platinum test-wire falls, since no provision is made in this case to restore the initial temperature. A calibration curve has therefore to be prepared for a given exciting current, whereby the readings of the voltmeter, which may be a recording instrument, become convertible into wind-velocities. A set of calibration curves is given in Fig. 14 to the left-hand scale, both for 1.5 amperes and 2.0 amperes, constantly sup-

plied to the test-wire from the 110-volt circuit. It will be seen that when the wire is greatly cooled by the wind, a very appreciable correction for the temperature of the wind enters into the result; although at a temperature elevation of say 300° C, this correction would be comparatively small. It is necessary to set the current at such a value that when the wind fails, the test-wire shall not be dangerously heated. It was found that with a platinum test-wire of 0.114 mm. diameter, as used in these measurements, 1.5 amperes was a suitable current for wind-velocities up to 15 km. per hour. Thus, as indicated in Fig. 14, with a wind of 10 km. per hour, and a temperature of 10° C., the voltmeter reading was 4.3 volts. When, however, the wind-velocities were higher, the current was increased to 2 amperes, which, in still air, raised the wire temperature to visible redness. At 30 km. per hour, and 10° C. wind temperature, the p.d. on 15 cm. was then 5.8 volts.

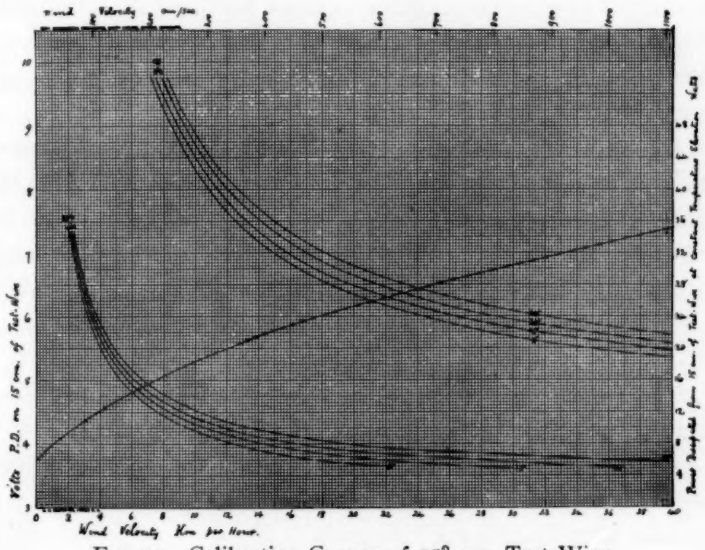


FIG. 14. Calibration Curves of 15° cm. Test Wire.

The *G*-support and test-wire were fastened to a pole and supported out of doors, exposed to the weather for some weeks. The apparatus appeared to be durable, and constant in its indications throughout that time, except that the solderings of the platinum tap-

wires across the test-wire, introduced an element of weakness. After exposure of some days, the test-wire was apt to break at a soldered point. In subsequent out-door trials, therefore, the tap-wires were simply twisted on to the test-wire. These tap-points involve a certain small error due to their cooling effect on the test-wire. It is, therefore, desirable to make the tap-wire as thin as is practicable, in view of both mechanical strength and electric resistance.

In a wind, the readings of the voltmeter anemometer are always fluctuating; but a mean value at any moment can always be estimated. A heavily damped voltmeter may conveniently be used.

The rising parabolic curve in Fig. 14 indicates, to the right-hand scale, the watts convected from 15 cm. of the same test-wire at different wind-velocities up to 1,100 cm./sec. or 40 km./hr. with constant resistance maintained in the wire; *i. e.*, by the method of Fig. 12. It is evident that for high wind-velocities, the indirect method of constant test-wire temperature is more sensitive than the direct-reading method of constant current. For lower velocities, however, the direct-reading method is much the more convenient, but requires to be corrected for the wind-temperature.

In the use of the constant-current direct-reading method, tungsten lamps have an advantage for the regulating resistance of Fig. 13 in that they tend to compensate for changes in the resistance of the test-wire at different wind-velocities; or to act as ballast resistance for the closer maintenance of constant current.

Conclusions.

1. The forced convection of heat from a thin platinum wire at constant temperature moved transversely through the air, varies not only approximately as the square root of the speed, but also approximately as the square root of the air-pressure (or air-density), in accordance with Boussinesq's formula.

2. At air-pressures below half an atmosphere, the above square-root relation was sensibly departed from. At pressures above 3 atmospheres, the relation was also departed from. Although the departures in both cases exceed the limits of observation errors, it is not certain whether they may have been due to imperfections in the

apparatus, such as eddy-currents in the confined air, caused by air-churning in the pressure-tank. In other words, it is uncertain whether, if a more spacious pressure-tank could have been used, with a longer fork radius, the square-root relation might not have held better than it did at the upper and lower air-pressures.

3. The effect of moisture in the air, upon the forced convection of heat from a thin wire, seems to be small, and has not yet been determined.

4. The forced convection of heat from a thin wire at constant temperature in air of variable pressure may be regarded as varying approximately with the square root of the mass of air displaced by the wire per second, or as $\sqrt{v\sigma}$, where v is the wind-velocity of displacement and σ the density of the air.

5. When the air-pressure p remains constant, $(P_c/\theta)^2$ or the square of the linear convection per deg. C. elevation, plotted against wind-velocity v, gives a straight line for a thin metallic wire. When, however, the air-pressure p varies, then it seems that, at least to a first approximation, $(P_c/\theta)^2/p$ plotted against wind-velocity v, gives one and the same straight line, for that wire, for all values of p from 0.5 to 2.0 megabars.

6. A thin vertical platinum wire about 25 cm. long, after being calibrated in a motor-driven fork, can serve as a recording wind measurer or anemometer. The record requires to be corrected both for the temperature and pressure of the air. The degree of precision obtainable is greatest at low wind-velocities.

THE UNITED STATES AS A FACTOR IN WORLD POLITICS.

By L. S. ROWE, PH.D., LL.D.

(*Read April 24, 1914.*)

There is probably no characteristic of American political life that has made a deeper impression on foreign students of our institutions than the fact that while all matters of domestic policy are subjected to the most searching analysis, questions of foreign policy do not in normal times become the topic of public discussion. It is not until our international relations have reached a critical stage that real public interest is aroused. In most cases, however, before public opinion has become crystallized, the national executive has committed the country to a certain line of action. Thereafter the country's policy is determined by the logic of events rather than by the dictates of public opinion.

In comparing the French, German and British magazines and newspapers with those of the United States, one is impressed with the fact that while the European journals are constantly discussing questions of foreign policy, little or no attention is given to this subject in our American journals until some problem has reached so critical a stage that it imperils the peace and safety of the country. The causes of this contrast are to be found not in the more serious character of foreign journals, but in the fact that the American people have been accustomed to confine their thinking on public affairs to questions of domestic policy.

Under our system of government this attitude involves a real danger because it removes our foreign relations from the control of public opinion, and makes them dependent upon the personal views of the President of the United States, subject to such influences as may, for the time being, be dominant in Washington. When public opinion does assert itself, such assertion usually comes as the result of a wave of popular feeling, obeying an emotional impulse. The

root of the difficulty is that the national thought of the people of the United States has failed to keep pace with the changes in the international position of the country. The early policy of our country, as well as our national thinking during the infancy of the republic, was concentrated on the idea of national isolation—freedom from entangling alliances with European countries, and the separation of American from European interests. National thought has remained in this stage of development whereas national power and national influence have long advanced beyond these narrow confines. We have attained the dignity of a world power, but our national thought has not advanced to a consciousness of the responsibilities which this position involves. The great problem now confronting the country is to bring about closer harmony between these two factors.

We are at the present moment witnessing one of the most serious consequences of this lack of adjustment which is affecting the international position and influence of the United States to a degree which cannot help but arouse the grave concern of every thoughtful and patriotic citizen. In a brief period of fifteen years we seem to have sacrificed the position of leadership in the maintenance of world peace, and have become one of the disturbing factors in international affairs. How is it, it will be asked, that a nation which through the contributions of more than a century has gained an enviable position as a leader in the great movement for the advancement of international goodwill, should within so short a space of time sacrifice this enviable position and come to be looked upon by all nations of western civilization as an uncertain factor in the orderly development of international relations.

Every student of international law and of world politics has been deeply impressed by the important part played by the United States in placing the conduct of international relations on a distinctly higher plane. It seems, at first glance, extraordinary that during the first half century of its existence a nation so weak and in many respects so unorganized should have been able to exert so important an influence on international law. When, however, we stop to reflect that during the first decades of the nineteenth century the United States held the balance of power amongst the nations of western civilization, the apparent paradox is readily explained.

The farseeing statesmanship of the founders of the republic led to the adoption, as a cardinal principle of American foreign policy, that the United States must be kept free not merely from entangling European alliances, but from any participation in the conflicts then raging in Europe. This principle of aloofness from European entanglements led to the assertion of those principles of American neutrality which, while serving primarily the interests of our national integrity, accomplished the still larger purpose of laying the foundations for the modern law of neutrality. In performing this service the United States contributed toward eliminating some of the most fruitful causes of international irritation, thereby promoting the interests of world peace.

It has been the laudable ambition of successive secretaries of state to continue and to strengthen those traditions which gave to the country a position of such unique power amongst the nations of both eastern and western civilization. In spite of these efforts, however, there is noticeable during recent years a distinct falling off in our international prestige. Little by little, the confidence of the peoples of Europe and of the American Continent has been undermined until to-day we find ourselves in a situation which possesses none of the elements of that splendid isolation which so long characterized the position of Great Britain and which, if not remedied, is likely to deprive us of the possibility of carrying to a successful conclusion a mission which constitutes the chief glory of American foreign policy during the first century of our national existence. It is, therefore, a matter of real national moment to inquire into the causes which have brought about this change, and to seek a remedy if such exists.

Of the elements contributing to the present situation, some are of long standing, the cumulative effects of which are now being felt, while others are of comparatively recent development. Amidst the splendid record of achievement during the first century of our national existence there looms up one aspect of our policy which has been a cause of deep concern to successive presidents and to successive secretaries of state. I refer to the inadequacy of our national legislation for the protection of resident aliens. A long

series of massacres, beginning with the Chinese massacre at Rock Springs, Wyoming, in 1895 and ending with the lynching of Italians in 1899, 1901 and 1910, have placed our national government in the humiliating position of acknowledging to foreign powers that although the sole responsibility for the conduct of our foreign relations rests with the federal authorities, they lack the power to fulfill the most fundamental of international obligations—the duty to bring to justice the persons responsible for such crimes. The matter was referred to as early as 1899 by President McKinley, who, in his annual message of December 5th, said:

"For the fourth time in the present decade a question has arisen with the government of Italy in regard to the lynching of Italian subjects. The latest of these deplorable events occurred at Tallulah, Louisiana, whereby five unfortunates of Italian origin were taken from jail and hanged. . . . The recurrence of these distressing manifestations of blind mob fury directed at dependents or natives of a foreign country suggests that the contingency has arisen for action by Congress in the direction of conferring upon the Federal courts jurisdiction in this class of international cases where the ultimate responsibility of the Federal Government may be involved."

The matter was again vigorously taken up by President Roosevelt in his message of December, 1906, in which he said in referring to the difficulties that had arisen because of educational discrimination against the Japanese in California:

"One of the great embarrassments attending the performance of our international obligations is the fact that the statutes of the United States are entirely inadequate. They fail to give to the national government sufficiently ample power, through United States courts, and by the use of the army and navy, to protect aliens in the rights secured to them under solemn treaties which are the law of the land. I, therefore, earnestly recommend, that the criminal and civil statutes of the United States be so amended and added to as to enable the president, acting for the United States government, which is responsible in our international relations, to enforce the rights of aliens under treaties. There should be no particle of doubt as to the power of the national government completely to perform and enforce its own obligations to other nations. The mob of a single city may at any time perform acts of lawless violence against some class of foreigners which would plunge us into a war. That city by itself would be powerless to make defense against the foreign power thus assaulted, and if independent of this government it would never venture to perform or permit the performance of the acts complained of. The entire power and the whole duty to protect the offending city or the offending community lie in the hands of the United States government. It is unthinkable that we should continue a policy under which a given locality may be

allowed to commit a crime against a friendly nation, and the United States government limited, not to preventing the commission of the crime, but, in the last resort, to defending the people who have committed it against the consequences of their own wrong doing."

In 1909, in his inaugural address, President Taft emphasized this serious defect in the conduct of our foreign relations, in the following words:

"By proper legislation we may, and ought to place in the hands of the federal executive the means of enforcing the treaty rights of such aliens in the courts of the federal government. It puts our government in a pusillanimous position to make definite engagements to protect aliens, and then to excuse the failure to perform those engagements by an explanation that the duty to keep them is in states or cities, not within our control. If we would promise we must put ourselves in a position to perform our promise. We cannot permit the possible failure of justice, due to local prejudice in any state or municipal government, to expose us to the risk of a war, which might be avoided if federal jurisdiction was asserted by suitable legislation by Congress and carried out by proper proceedings instituted by the executive in the courts of the national government."

It is clear that no nation can shirk the responsibilities of its international obligations without arousing widespread opposition. The constitutional authority granted to our federal government is sufficiently comprehensive to include all powers necessary to meet our international obligations. We cannot permit our states, which occupy no international status, to plunge us into irritating controversies with foreign countries. The dignity of the national government and the demands of national self-respect require that the federal executive be given statutory powers sufficiently broad and that the federal judiciary be given jurisdictional authority sufficiently comprehensive to enable the national government to do its full duty in the protection of the person and property of aliens resident within our borders. The first step in this direction is the enactment of a law giving to the federal courts jurisdiction over all cases in which the treaty rights of a citizen or subject of a foreign country are involved. A bill to this effect has been before the Congress of the United States on several different occasions. Its precise text is as follows:

"Be it enacted by the Senate and House of Representatives of the United States of America, in Congress assembled, that any act committed in any state or territory of the United States in violation of the rights of a citizen

or subject of a foreign country secured to such citizen or subject by treaty between the United States and such foreign country, which act constitutes a crime under the laws of such state or territory, shall constitute a like crime against the peace and dignity of the United States, punishable in like manner as in the courts of said state or territory, and within the period limited by the laws of such state or territory, and may be prosecuted in the courts of the United States, and upon conviction, the sentence executed in like manner as sentences upon convictions for crimes under the laws of the United States."

That the federal government has ample power to enact such legislation has been repeatedly affirmed by the Supreme Court of the United States. In Baldwin *vs.* Frank (120 U. S. 678) one of the leading cases on the subject, the question involved was whether the Civil Rights Acts were applicable to a conspiracy to deprive Chinese subjects, residing within a state, of rights secured to them by treaty. In the course of its opinion the court said:

"The precise question we have to determine is not whether Congress has the constitutional authority to provide for the punishment of such an offense as that with which Baldwin is charged, but whether it has so done.

"That the treaty-making power has been surrendered by the states and given to the United States is unquestionable. It is true also that the treaties made by the United States and in force are part of the supreme law of the land, and that they are as binding within the territorial limits of the states as they are elsewhere throughout the dominion of the United States.

"That the United States have power under the Constitution to provide for the punishment of those who are guilty of depriving Chinese subjects of any of the rights, privileges, immunities or exemptions guaranteed to them by this Treaty, we do not doubt. What we have to decide, under the questions certified here from the court below, is whether this has been done by the sections of the Revised Statutes specially referred to."

Again, in the Debs case (158 U. S. 564) the Court held:

"The entire strength of the nation may be used to enforce in any part of the land the full and free exercise of all national powers and the security of all rights entrusted by the Constitution to its care. . . . If the emergency arises, the army of the nation, and all its militia, are at the service of the nation to compel obedience to its laws."

All this tends to prove not only that we have been remiss in the performance of our international obligations, but that such remissness has not been due to any defect in our national Constitution but to the failure of Congress to extend the jurisdiction of the federal courts and to grant specific authority to the federal executive to

fulfill manifest international obligations. Indeed, it would seem from the decision in the Debs case that even without specific statutory authority, the national executive may do far more than has hitherto been done in maintaining the supremacy of the federal treaty making power.

The failure of Congress to make adequate provision for the protection of resident aliens has aroused resentment not only in the states whose *nationals* have suffered most severely, but has seriously injured our reputation for fair dealing throughout the civilized world. The remedy for this situation is so simple that there is no excuse for further delay in making it effective.

A second influence which has played an important part in estranging the goodwill of foreign countries is the widespread belief that there exists in the Congress of the United States a tendency to force upon the executive a narrow and technical interpretation of treaties. Secretary Hay once said of certain senators who attempted to defeat every treaty presented to the Senate, that their idea of a treaty was a document which gained everything for the United States and gave nothing to the other party. The ruthless way in which the Congress of the United States has at times swept aside treaty obligations, and the unwillingness to bring national legislative policy into harmony with our international obligations, have created the impression that the promises of the United States cannot be depended upon, and that even the best intentions of the President and his advisers are apt to be thwarted by the action of Congress.

The culminating point of a series of instances was reached in the provision of the Panama Canal Act exempting American coastwise shipping from the payment of canal tolls. Whatever may be our opinion as to the desirability of the exemption clause viewed as a question of domestic policy, it is clear from the history of the Clayton-Bulwer and of the Hay-Pauncefote treaties and from the testimony of those who assisted in their negotiation that the United States made no attempt to reserve to itself the right to give preferential treatment to its own merchant vessels. The privileges acquired by the United States under the Hay-Pauncefote Treaty involved certain concessions on the part of Great Britain, for which she ex-

acted the observance of the principle of equality of treatment. It would be a reflection on our country's reputation for fair dealing if, after securing the abrogation of the Clayton-Bulwer Treaty, we were to repudiate the concessions, the making of which rendered possible the ratification of the Hay-Pauncefote Treaty.

It is fortunate for the world's peace that there is rapidly developing a body of international opinion to which the policy and conduct of individual nations must conform. Violations of the standards set by this opinion place the offending nations under the ban of international disapproval. With each year the commercial and social relations between nations are becoming closer. This increasing interdependence means that national policy must be made subservient to international right and to international obligation. No nation is a law unto itself, and it is evident that even our concept of national sovereignty must be subjected to revision in order to conform more closely to those larger principles of international reciprocity and fair dealing, upon which the maintenance of western civilization so largely depends. Just as competition has gradually given way to coöperation in the industrial world, so in international affairs the concerted action of states and the idea of mutual obligation as between states are gradually taking the place of the more primitive principle that every nation may formulate its national policy on the basis of national interests regardless of the higher standards of conduct now prevailing in the society of states.

Fortunately, for the good name of the United States, the President has courageously taken a position which has not only aroused the admiration of the civilized world but has placed our country under a debt of obligation. In his address of March 5, 1914, to the Congress of the United States he sounded a note which served to impress upon the nation the sacredness of treaty obligations. Speaking of the Hay-Pauncefote Treaty he said:

"We consented to the treaty; its language we accepted, if we did not originate it; and we are too big, too powerful, too self-respecting a nation to interpret with a too-strained or refined reading the words of our own promises just because we have power enough to give us leave to read them as we please. The large thing to do is the only thing we can afford to do, a voluntary withdrawal from a position everywhere questioned and mis-

understood. We ought to reverse our action without raising the question whether we were right or wrong and so once more deserve our reputation for generosity and for the redemption of every obligation without quibble or hesitation."

The magnitude of the President's service goes far beyond the vindication of the Hay-Pauncefote Treaty. These words and the determination which lies back of them place the international relations of the United States on a distinctly higher plane, and, if properly supported by the united opinion of the country, will do much toward regaining for the United States the enviable position which we once occupied. All secondary and party interests must be made to bow before that higher standard of international dealing which the President so vigorously champions.

A third influence which has played an important part in arousing opposition to the United States has been the tendency to permit new doctrines of American foreign policy to masquerade under the cloak of the Monroe Doctrine. In the adjustment of our relations with Mexico, with the islands of the West Indies, and with the countries of Central America, we have fallen into the error of endeavoring to build up our relations on the basis of the negative principles formulated in 1823. Instead of clearly and definitely facing the fact that these sections of the American continent occupy an exceptional relation toward the United States, and building up our policy on the basis of this exceptional relationship, we have formulated vague principles purporting to be based on the Monroe Doctrine which have aroused the suspicion, the distrust, and even the hostility of the most progressive countries of South America. We should clearly and definitely recognize the fact that everything that affects the progress, the stability and the well-being of the islands of the West Indies, of Mexico and of Central America, is a matter of immediate and direct concern to the United States. It is a concern different in kind from that which affects our relations with the countries of South America. The acquisition of Porto Rico and our exceptional relations with Cuba, have made of the United States a West Indian power. The construction of the Panama Canal and the acquisition of the Canal Zone have made of the United States a Central-American power, and finally, the fact that Mexico is our

neighbor, that large bodies of American citizens have taken up residence there, and that vast American interests amounting to over a billion dollars, are invested in Mexican enterprises—all these factors indicate the necessity of developing a policy toward these three sections of the American continent based on the positive national interests involved rather than on the negative principles of the Monroe Doctrine. We should frankly proclaim to the world that basic national interests demand that these sections of the American continent shall not only remain free from European complications, but that the primary requisites for the preservation of civilization shall be maintained. Continued disorder, the disregard of fundamental human rights, the undermining of respect for property—all these constitute elements which vitally affect the well-being and safety of the United States. This does not mean either the extension of a proctectorate or an unwarranted interference with the domestic affairs of these countries, but it does mean that the United States cannot remain indifferent to the existence of conditions which menace the fundamentals of civilization.

The positive principles of foreign policy which this exceptional situation demands, rest in part on the fact that the sections of the American continent above referred to have become important sources of the food supply of the American people, and that the possibility of reducing the cost of living of the American workingman depends in large measure on the uninterrupted use of these sources of supply.

This does not mean that the United States should pursue a selfish or ruthless policy in dealing with these countries. On the contrary, the permanent interests of the United States are best subserved by prosperous, independent, self-respecting and progressive neighbors. Our policy toward them should be conceived in the most elevated spirit of helpful coöperation, but the basis of this coöperation should be and must be the maintenance of the fundamental requisites of civilization.

The Monroe Doctrine was formulated to accomplish two specific purposes:

1st. To prevent further European colonization on the American continent, and

2d. To prevent the extension of the European political system to the United States, the overthrow of the domestic institutions of an American state by European influence, or the control of the political destinies of an American state by any European power.

These two leading principles, which were of an essentially negative character are still vital principles of the foreign policy of the United States, in the maintenance of which every state of the American continent is deeply interested. The cordiality of our relations with the countries of South America demands that the Monroe Doctrine be limited to its original content. If this is done, there need be no fear of wounding the sensibilities or arousing the opposition of the countries of South America.

When, therefore, in our relations with the islands of the West Indies, with Central America or with Mexico, it becomes necessary to go beyond the negative principles of the Monroe Doctrine, and enforce positive principles of foreign policy, let us take such a step fully cognizant of the fact that we are not acting under any supposed principles of the Monroe Doctrine, but on the basis of a policy dictated by the requirements of the special conditions in the Mediterannean section of the American continent. Special agreements looking toward coöperation for the maintenance of stability, such as the reorganization of the San Domingan finances, do not rest on any principle of the Monroe Doctrine, but are dictated by the exceptional relationship above referred to.

Our Mexican policy is another of the influences that has reacted unfavorably upon the international position of the United States. No one will doubt for a moment the lofty ideals which have actuated the President in the formulation of his policy, but it is also clear that in spite of all protestations to the contrary our Mexican policy has aroused a marked feeling of opposition amongst the peoples of Central and South America, and has served to foster secret understandings between European governments for the purpose of protecting what they regard as their national rights and the rights of their citizens. We must always bear in mind that whatever may be our personal views with reference to the Monroe Doctrine it has always been regarded by the European countries as an expression

of the intention of the United States to reserve the countries of the American continent as a special field for its commercial and industrial influence.

The unwillingness of the United States to allow European governments to intervene in Mexico for the protection of the interests of their citizens and subjects, combined with the reluctance of the United States to accept the full responsibilities which this position involves, has served to accentuate the feeling of opposition to the United States which has been growing so rapidly within recent years.

Furthermore, the attempt on the part of the President to deal with the Mexican situation as if it were part of a general Latin-American problem instead of facing it squarely as a problem *sui generis,* involving an exceptional relationship between neighboring countries, has aroused the bitter opposition of the countries of Central and South America. The insistence of the United States on the retirement of a provisional President is looked upon as a form of unwarranted dictation, and as an indication of a settled purpose on the part of the United States to assert a kind of political supervision over the republics of the American continent.

In conclusion, I desire to refer to a recent occurrence which has given rise to serious misgivings both in Europe and in the countries of Central and South America. In an address delivered before the Southern Commercial Congress, the President of the United States announced a new principle of American foreign policy, the purpose of which seems to be the gradual financial emancipation of the countries of Central and South America from their present dependence on European capital. In the course of this address, the President attacked

> "the material interests that had influenced the foreign policy of certain Governments in their relations with the nations of Latin-America."

He declared it to be the duty of the United States,

> "to assist the nations of this hemisphere in their emancipation from the material interests of other nations, so that they might enjoy constitutional liberty unrestrained." "You hear," he said, "of concessions to foreign capital in Latin-America . . . States that are obliged to grant concessions are in the position that foreign interests are apt to dominate their affairs. Such a state of things is apt to become intolerable. It is emancipation from this inevitable subordination that we deem it our duty to assist."

It is true that the President restricted himself to a declaration against "concessions," and it would seem that to his mind this term involves the idea of special privilege or monopoly. The nearest English equivalent of the Spanish word "*concesión*" is our own legal term "franchise." It is true that in many of the countries of Central and South America such franchises include the grant of monopolistic privileges. It is also true that under the cloak of such franchises many abuses have been committed, but we must bear in mind that the unsettled political conditions prevailing in many of these countries and the exceptional risks to which foreign capital is subjected, have made it necessary to offer exceptional inducements in order to attract foreign investors. If we stop to reflect on the extraordinary inducements which were offered to foreign capital during the early history of the United States, and on the great service which such capital rendered to our national development, we can readily see that any policy, the effect of which is to discourage foreign investments in Central and South America, cannot help but retard the development of those sections of the continent. We may deplore the fact that in many of the republics of the American continent there has been a wasteful and at times a corrupt distribution of franchises and special privileges, but it is a serious question whether it is either our duty or our right to undertake to determine or even to suggest the standards or conditions to which the investment of foreign capital should conform.

At all events, let us not close our eyes to the fact that the formulation of this policy has aroused serious misgivings throughout the countries of the American continent, as it is looked upon as an unwarranted assumption of control over their liberty of action. In Europe the President's pronouncement is regarded as confirmatory of a suspicion, which has been growing within recent years, namely that the United States has embarked upon a national policy, the purpose of which is to reserve the less advanced countries of the American continent for the economic exploitation of American capital.

Whatever the ultimate judgment on the appropriateness of the principles or the wisdom of the policy formulated in the President's Mobile speech, it should be made clear that this new orientation of

our foreign policy is not a part of the Monroe Doctrine, and has no organic relation to the fundamental principle upon which the Monroe Doctrine rests, namely, national safety and self-protection. It is a new and strange principle which has aroused the opposition of the countries for whose benefit it is intended, and has engendered bitterness of feeling amongst European peoples. If it is to be maintained it must justify itself by basic reasons of national interests and international obligation entirely independent of the Monroe Doctrine.

These, in brief, are the more important influences that have aroused the opposition of many sections of the American continent and the animosity of Europe, and have placed the United States in a position of international isolation. It would be idle to argue that we have been the victim of circumstances because, as we have seen, the position in which the United States finds herself at the present time is traceable to the fact that our national thought and national consciousness have not kept pace with our international responsibilities. The most serious aspect of this condition of isolation is that it prevents us from fulfilling the high mission in international affairs which, by reason of our exceptional geographical position, by reason of our exceptional relationship to European as well as to American affairs, we are manifestly called upon to perform. The words spoken by Mr. Root at the Fourth Pan-American Conference at Rio Janeiro set the standards which should ever remain before the American people:

> "We wish for no victories but those of peace; for no territory except our own; for no sovereignty except the sovereignty over ourselves. We deem the independence and equal rights of the smallest and weakest member of the family of nations entitled to as much respect as those of the greatest empire, and we deem the observance of that respect the chief guaranty of the weak against the oppression of the strong. We neither claim nor desire any rights, or privileges, or powers that we do not freely concede to every American Republic. We wish to increase our prosperity, to expand our trade, to grow in wealth, in wisdom and in spirit, but our conception of the true way to accomplish this is not to pull down others and profit by their ruin, but to help all friends to a common prosperity and a common growth, that we may all become greater and stronger together."

THE MORPHOLOGY OF THE PASSAMAQUODDY LANGUAGE OF MAINE.

By J. DYNELEY PRINCE.

(*Read April 25, 1914.*)

The Passamaquoddy Indians of Maine, together with the Maliseets (Milicetes) or St. John's River Indians of New Brunswick form a single linguistic group of the eastern Algonquin family known as *Wabanaki,*' people of the dawnland,' or 'East.' It is estimated that there are about three hundred and fifty people in each clan. The other members of the group are the Micmacs of Nova Scotia, the Abenakis (a corruption of Wabanaki) or St. Francis Indians of Quebec, and the Penobscots of Oldtown, Maine, the two latter clans also forming a linguistic group similar to that of the Passamaquoddies and Maliseets.[1] The name "Passamaquoddy" is a corruption of *pestumo'kat,* 'one who catches pollack fish' (*Gadus Pollachius*) = *peska'tum.* This term has been applied to the tribe only in comparatively recent times. The headquarters of the Passamaquoddies are at Pleasant Point, Maine (*Sipáyik*), where the remnants of the tribal organization still exist. Here, for example, dwelt Sopiel Selmo, the keeper of the Wampum Record, a mnemonic system of wam-

[1] For the Wabanaki group, cf. my articles: "Notes on the Language of the Eastern Algonquin Tribes," *Amer. Jour. Philol.,* IX., pp. 310–316; "The Wampum Record," *Proc. Amer. Philos. Soc.,* 1897, pp. 479–495; "Forgotten Indian Place-names in the Adirondacks," *Jour. Amer. Folk-Lore,* 1900, pp. 123–128; "Some Passamaquoddy Witchcraft Tales," *Proc. Amer. Philos. Soc.,* XXXVIII., pp. 181–189; "Notes on Passamaquoddy Literature," *Annals N. Y. Academy of Sciences,* XIII., pp. 381–386; "The Modern Dialect of the Canadian Abenakis," *Miscellanea Linguistica in Onore di Graziodio Ascoli,* 1901, pp. 343–362; Leland and Prince, "Kulóskap the Master," Funk and Wagnalls, New York, 1902; "The Algonquin Noun," Proceedings of the Congress of Orientalists, Rome, 1904; "Algonquin Religion," Hastings, Dictionary of Religions, s. v. "God"; "A Micmac Manuscript," Proceedings of the Congress of Americanists, Quebec, 1908; "A Passamaquoddy Aviator," *Amer. Anthropologist,* XI.

pum shells arranged on strings in such a manner, that certain combinations suggested certain sentences or ideas to the narrator, who, of course, knew his record by heart and was merely aided by the association of the shell combinations in his mind with incidents of the tale, song or ceremony which he was rendering (Prince, *Proc. Amer. Philos. Society*, December 3, 1897, pp. 479–495). With Selmo, however, died the secret of this curious system, but the laws and customs thereby recorded have been preserved and published in the *Proceedings of the American Philosophical Society* (*loc. cit.*). There is also a large amount of oral literature handed down by these Indians, a quantity of which exists in the manuscripts of the Hon. Lewis Mitchell, Indian member of the Maine Legislature. These documents are now in my possession and I expect to publish their material in an exhaustive work on the Passamaquoddy tribe and language. Some matter of this character has already appeared, both in the *Proceedings* of this Society (XXXVIII., pp. 181–189), and also in Leland and Prince, "Kulóskap the Master," New York, 1902, a popular exposition of the eastern Algonquin folk-lore.

The object of the present paper is to discuss briefly the chief peculiarities of the Passamaquoddy idiom, as it is now in use. No detailed presentation of the morphology of this dialect has been made as yet, although some of its features have been noticed. Nearly all the materials for the present article have been gathered orally from the Passamaquoddies and tested by means of the Mitchell manuscripts.

PHONETICS.

The phonetics of the dialect are comparatively simple. The system followed herein gives to the vowels the Italian pronunciation, except *â* = English *aw* in 'awful' and the indeterminate vowel (*Schwund*) which is indicated by the apostrophe '. There are no nasal vowels in Passamaquoddy, as in Abenaki. *H* has the value of the simple breathing, but the inverted comma ' is the well known Algonquin glottal catch, pronounced like a very soft Arabic medial *He*. *Ch* often represents a palatal ts^y and between vowels has a tendency to approach *j* = ds^y. The constants *p*, *t*, k are voiceless surds often approaching *b*, *d* and hard *g* between vowels, but never

uttered like the English *p(h)*, *t(h)*, *k(h)*. *S* between vowels is sometimes occasionally pronounced almost *z*. The combination *w'* is the whistle peculiar to the Algonquin languages occurring at the end of syllables: *ke'kw;* pronounced *keḥkwu*. *W* in general is a weak consonant; it is almost equivalent to the *u*-vowel. It generally disappears after any prefix, as seen in the paradigm of *wikwus: nikwus; kikwus,* etc. It should be noted that the *l* carries an inherent vowel and is consequently pronounced very like the Polish barred *l*. *N* before consonants, as in the syllables *nta-; nki-* is pronounced with its own inherent vowel *'n*. The spelling of the Mitchell manuscripts is a mixture of English and French influences, frequently using *b, d, g* for *p, t, k; j* for *ch* and an arbitrarily varying system of vocalization. No attempt has been made to follow it in this paper.

The Passamaquoddy is not especially rich in consonantal clusters, although more so than some of the other Algonquin idioms, as for instance the Fox. The following table covers nearly all the consonantal groupings which appear, and even some of these are not true clusters, as they occur in many cases with vocalic *w* and *y*. The Indian manuscripts show many apparent clusters, but they are often really separated by the *Schwund* which no Indian ever writes. Thus clusters with *l, m, n* are suspicious, owing to the probability of inherent vowels. The *Schwund* and inherent vowels may be represented by *e*.

CLUSTERS.

chw: echwéchi, 'must.'
chy: apachyáie, 'when I come.'
k'm: w'-mikmaupaul'tínia, 'they feast together.'
ks: ěyiks, 'when they are' ('were').
kskw: chwopnokskwâk'n, 'anchor.'
kt: w'máche-m'siktihîk'n, 'he begins to punish him,' but here the *k* probably represents *k'*.
kw: akwét'n, 'canoe.'
kw'p: nisukmékw'p'n, 'as we (paddled) together.'
lk: túlkiu, 'thus' (real).
lh: amalhi'takwuk, 'they make music'; *hel'* 'fawn.'
lkw': alkw'minaútikuk, 'in their midst' (suspicious).

lp: hólpin, 'he sits.'
lt: -toltîtit = participial ending 3 p. pl. (real).
mk: el-ámkikap, 'when they came ashore.'
mkw': kikúmkwuk, 'suckers' (fish).
ms: n-olámsittam'n, 'I pretend to': *lams-,* 'blow.'
msk: ámskowás, 'first.'
nk: (nasal): *k'tunkian,* 'when you have hunted' (real).
nk: ankwoch, 'sometimes' (real).
ns: nsámakwán, 'water' (*n* with inherent vowel: *'n*).
pk: apkulámsek, 'they are overwhelmed by the wind.'
pkw': achápkwalusk, 'perch' (fish).
pn: chwopnokswâk'n, 'anchor' (not *p'n*).
ps: ásups, 'grebe' (sort of crane).
psk: ápskeduk, 'than.'
pt: appatáptow'k, 'they come back'; *náchipt,* 'fetch' (imper.).
pw: apwitowatíchil, 'they stand.'
sh: apakweshes, '*red-headed* wood-pecker' (= *s-h;* not the English *sh*).
sk: mosk, 'find.'
skw': peskw, 'one.'
sm: iklísm'n, 'white man' = 'Englishman'; *ismékwes,* 'fish-hawk.'
sn: p'snut, 'basket.'
sp: kúspem, 'lake'; *spāswĕu,* 'morning.'
st: chestésit, 'he being angry'; *stáknut,* 'green.'
sw: k'loswâk'n, 'word.'
th: petholatíchihi, 'when they arrive.'
tk: atkéyi, 'string, cord'; *uskitkamikw,* 'world.'
tm: chepakatm'n, 'that you shall marry.'
ts: mits, 'eat' (imper.).
tsk: mitskun, 'animal dung.'
tw: lakutwâk'n, 'treaty.'

The intonation of the Passamaquoddy is highly tonic. Almost every accented syllable indicates a voice-lift. The voice is dropped on the syllable following the tone; half raised again on the third syllable and dropped again on the fourth: *lakutwâk'n;* pronounced $^{la}kut^{wâ}k'n$. This peculiarity appears to be distinctively Passama-

quoddy, as the kindred Maliseets speak very monotonously and with no especially noticeable voice-lift on the accented syllable. The Abenakis also have a monotonous tone, amounting practically to a drawl.

MORPHOLOGY.

In Passamaquoddy, as in all the other Algonquin idioms, the words are really only indifferent themes, which may be used either in a nominal or a verbal sense. This phenomenon is too well known to require elaboration, but it may be illustrated by the appearance of such indeterminate roots in all the parts of speech; thus, *sak*, 'be strong, rule'; as seen in *w't-ach'wi-saki'tón'l*, 'he must rule'; *saklḗyo*, 'it is hard, difficult'; *saklikwáso*, 'it is served up strong'; *sákem*, 'chief'; *sak'mawámat*, 'he who is chief'; *k'ti-sak'mawé'lul*, 'I wish to make you chief'; *sák'ma-wíkwām*, 'chief's house'; *sak'mawel-mékw'tech*, 'may it be hallowed'='made like a chief,' etc. Similarly, the root *os*, 'shine,' appears in *kís-os*, 'luminary' (probably='what can [*kis-*] shine'); hence='moon, month'; *el-āse*, 'what shines'; *nip-aúsit*, 'what shines at night'; specifically 'the moon' (*nip*='night'), etc. (see just below on *w'li*).

The Substantive.

All substantives, including adjectival formations, are divided into two classes, animate and inanimate, no attention being paid to sex-gender, which is expressed either by prefixing or suffixing some determining word, or else by the use of a distinct expression; *kínchemés*, 'king' (literally: 'King James,' the name of the first king whose name the Indians heard); but *kínchemes-iskwé*, 'queen,' with the feminine suffix *-iskwé* (='squaw'); *mûin*, 'bear,' but *nápeskw*, 'she-bear'; *pût'p*, 'whale,' but *skwemé'kw*, 'she-whale,' etc. It should be noted that many substantives which we should regard as inanimates are treated as animates in Passamaquoddy and conversely; as *akim*, 'snow-shoe' (an.); but *sat-y-il*, 'blue-berries' (inan. pl.). Genuine inanimates are, *e. g.*, *t'm'hîk'n*, 'axe'; *wik-wam*, 'house,' and words of a similar character.

The plural ending for animate nouns is invariably *-k* and for inanimate *-l*, preceded as a rule by indeterminate connecting vowels;

mûs, 'moose'; pl. *mûs'k; ĕs,* 'clam'; pl. *ĕs'k.* Nouns ending in *-p* and dentals like *skí'tap,* 'man,' insert *-y-* and use the distinct ending *-ik;* pl. *skitápyik;* cf. also *pílchisáp-yil,* 'trouser-braces' (inan.); *piljis* = English 'breeches' + *ap,* 'hang'; lit. = 'hang-breeches.' Participial forms in *-t,* like *ĕpīt,* 'woman,' palatalize the *ty* into *ch;* pl. *ĕpichik* for *ĕpityik.* The same phenomenon of *y* after labials and dentals appears in *wikhîk'nepyĕi,* 'inkstand' (*wikhîk'n-nep,* 'book-water' + *-ei,* 'thing'), and in *met-y-ēwéstakw,* 'he is saying' (*met-* = prolonged action + *y* + *ēwest,* 'speak' + participial ending *-akw*). Note also *sat-y-il* (inan.), 'blue-berries.' I can find no trace of the connecting vowels *a, o* and *e* (*i*) used after specific consonants, as in Abenaki (cf. Prince, "Ascoli Memorial," p. 349). Sometimes, however, after *-s* a plural in *-w'k* occurs, as *kísos,* 'month'; pl. *kis'w'k,* but, on the other hand, *kakakûs,* 'crow'; pl. *kakakûs'k.* There seems to be a natural affinity in the labial and dental for the connecting *-y-*.

THE ADJECTIVE.

Adjectives may be used as separate indeclinable particles preceding the noun; as *k'chî skí'tap,* 'great man'; *sigi m'tápekwin,* 'fierce warrior.' They may also appear in this indeclinable form adverbially, as *kátamayíwi-k'túnkiyik,* 'while absent they hunt' (lit. 'not' = *katama* + *y,* connecter, + 'being' = *i-wi*). Very common is the use of adjectives ending in *-ko* for animates and *-k'n* for inanimates: *w'lîko skí'tap,* 'good man'; pl. *w'lîkow'k skitápyik; w'lîk'n wíkwām,* 'good house'; pl. *w'lîkn'l wíkwām'l.* The participial *-it* (an.) and *ek* (*ik*) (inan.) is a common adjectival ending as *w'lîkit,* 'handsome' (an.); *w'lîkek* (inan.). When the adjective is inflected, it must agree with its nouns, which it may follow or precede: *sāks'l picheyíkil,* 'long stockings' (= *sāks'l*). Some adjectives end in *-l,* as *nék'm'kil skí'tap,* 'big man,' which is not an obviative *l* (see below, Substantival Modifications, No. 1). Adjectives may be formed from nouns by the endings *-wi* as *skitáp-wi,* 'human,' and *-(k)ewi: wíkwamkéwi,* 'homelike.'

The element *w'lî-(oli-)* may also be used adjectivally without inflection, as *w'lî haás,* 'good horse'; *w'lî wíkwām,* 'good house,' and in innumerable combinations both nominal and verbal. Per-

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haps no better example of Passamaquoddy polysynthetic formation can be had than the following partial list of the combinations possible with *w'lî: w'lithátm'n,* 'he is glad of it'; *k-elwut,* 'good'; *w'lāmto,* 'he is good natured'; *w'lásewanwechiyánia,* 'they are pleased with him'; *w'laswélt'm'n,* 'he thanks for it'; *olilélm'len,* 'as I wish you well'; *olínm'n'l,* 'he rejoices at it'; *w'lápémkuk,* 'it benefits hims'; *uliotwâk'n,* 'benefit' (n.); *w'l-wíku,* 'he lives well'='is rich'; *w'tách'wi-w'l-ánkeyowáw'l,* 'they must take good care of him'; *w'lapĕwit,* 'beautiful'; *w'lapēwiū,* 'handsome man'; *w'leyów'l,* 'he treats her well'; *w'litham'l,* 'he consents'; *w'l-okhedím'k,* 'sport, game,' etc.

THE PRONOUN.

The demonstrative pronouns are *yut,* 'this' (nearer); *wut,* 'this'; *na,* 'that,' indeclinable; and *nit, nit,* 'that.' See below for the obviative inflection. The relative for both classes and numbers is *eli-,* followed by the participle: *eli-uskichínwit,* 'he who is an Indian'; *eli-meksit,* 'who finds.' Sometimes the relative is expressed by the participle alone: *askowaltíchik,* 'those who wait.' The interrogative pronoun is as follows: animate *wen,* 'who'; obv. *wen'l;* also='someone'; obviative pl.=*wenihi;* inanimate: *ke'kw,* 'what'; also='something'; pl. *ke'kw's'l,* 'some things, things.'

A highly important feature of the language is the combination of both nouns and verbs with personal pronouns, by which means most of the inflection is carried on. In the following table of personal pronouns, it will be observed that the Passamaquoddy, like its Algonquin congeners, has two first persons plural; an inclusive and an exclusive, the first of which implies that the person and persons addressed are included with the speaker, while the second form excludes the person or persons addressed; *i. e.,* the first='I, you' and 'they,' and the second='I' and 'they.'

Separable Personal Pronouns.

I, *nĭl.*	We (incl.), *kîl'n.*
Thou, *kĭl.*	We (excl.), *nîl'n.*
He, she, it, *nĕg'm.*	You, *kîl'wau.*
	They, *nĕg'mau.*

The primitive elements of the first, second and third persons are *n*, *k* and *w*(*o*), prefixed in the separable pronouns above to certain demonstrative elements which are practically identical in all the dialects. These elements *n*, *k*, *w*(*o*) may also be prefixed to nouns, to indicate possession, and to verbs, to denote conjugational inflection. It will be noticed, however, that the separable pronoun of the third person is represented by a demonstrative particle *neg'm* (Abenaki *ag'ma;* Delaware *nekama*). This has no connection with the *w*(*o*)-prefix, but is a combination of the ordinary demonstrative *n*(*a; ni*), 'that one' + the asseverative *ga*, seen in *gak* (*passim*), and the possessive *-m* (see below).

The following diagram will illustrate the Passamaquoddy method of combining the pronominal *n*, *k*, *w*(*o*) with the animate and inanimate forms of nouns in order to denote the possessive relation.

DEF. AN.

nimía níkwus, 'I see my mother.'
k'nimía kíkwus, 'Thou seest thy mother.'
w'nimíal wikwús'l, 'He sees his mother.'
k'nimiánna kikwús'n, 'We (incl.) see our mother.'
nimianna nikwús'n, 'We (incl.) see our mother.'
k'nimiáwu kikwús'wu, 'You see your mother.'
w'nimiáw'l wikwús'w'l, 'They see their mother.'

DEF. INAN.

nimí'ton nt'm'hîk'n, 'I see my axe.'
k'nimí'ton k't'm'hîk'n, 'Thou seest thy axe.'
w'nimí'ton w't'm'hîk'n, 'He sees his axe.'
k'nimí'tonen k't'm'hîk'nen, 'We (incl.) see our axe.'
nimí'tonen n't'm'hík'nen, 'We (excl.) see our axe.'
k'nimi'tónia k't'm'hík'n'wu, 'You see your axe.'
w'nimi'tónia w't'm'hîk'n'wu, 'They see their axe.'

DEF. AN. PLURAL.

nimíuk nikwús'k, 'I see my mothers.'
k'nimîuk kikwús'k, 'Thou seest thy mothers.'

w'nimía wikwus, 'He sees his mothers.'
k'nimiánnawa kikwúsn'w'k, 'We (incl.) see our mothers.'
nimiánnawa nikwúsn'w'k, 'We (excl.) see our mothers.'
k'nimiáwu kikwús'w'k, 'You see your mothers.'
w'nimiáwu wikwusw'l, 'They see their mothers.'

DEF. INAN. PLURAL.

n'mi'ton'l nt'm'hîk'n'l, 'I see my axes.'
k'nimi'ton'l k't'm'hîk'n'l, 'Thou seest thy axes.'
w'nimi'ton w't'm'hîk'n'l, 'He sees his axes.'
k'nimi'tonén'w'l k't'm'hîk'nen'l, 'We (incl.) see our axes.'
nimi'tonén'w'l n't'm'hîk'nen'l, 'We (excl.) see our axes.'
k'nimi'ton'w'l k't'm'hîk'n'w'l, 'You see your axes.'
w'nimi'ton'w'l w't'm'hîk'n'w'l, 'They see their axes.'

INDEF. ANIMATE.

nimía wíkwus, 'I see a mother'; *wikwus'k,* 'mothers.'
k'nimía wíkwus, 'Thou seest a mother'; *wikwus'k,* 'mothers.'
w'nimía wikwús'l, 'He sees another'; *wikwus.*
k'nimiáp'n wíkwus, 'We (incl.) see a mother; *wikwus'k,* 'mothers.'
nimiáp'n wíkwus, 'We (excl.) see a mother'; *wikwus'k,* 'mothers.'
k'nimiá'pa wíkwus, 'You see a mother'; *wikwus'k* 'mothers.'
w'nimiánia wikwus'l, 'They see a mother'; *wikwus,* 'mothers.'

INDEF. INANIMATE.

nimi'to t'm'hîk'n, 'I see an axe'; *t'm'hîk'n'l,* 'axes.'
k'nimi'to t'm'hîk'n, 'Thou seest an axe'; *t'm'hîk'n'l,* 'axes.'
w'nimi'to t'm'hîk'n, 'He sees an axe'; *t'm'hîk'n'l,* 'axes.'
k'nimi'tonēp'n t'm'hîk'n, 'We (incl.) see an axe'; *t'm'hîk'n'l,* 'axes.'
nimi'tonēp'n t'm'hîk'n, 'We (excl.) see an axe'; *t'm'hîk'n'l,* 'axes.'
k'nimi'tó'pa t'm'hîk'n, 'You see an axe'; *t'm'hîk'n'l,* 'axes.'
w'nimi'tow'k t'm'hîk'n'l, 'They see an axe'; *t'm'hîk'n'l,* 'axes.'

The scheme of the possessive prefixes and suffixes for nouns is then as follows:

Animates.		Inanimates.	
Singular.		Singular.	
n——	*k*——*'n*		*k*——*en*
k——	*n*——*'n*	*n*——*'l*	*n*——*en*
w——*l*	*w*——*w'l*	*k*——	*k*——*'wu*
	w——*w'l*	*w*——	*w*——*'wu*
Plural.		Plural.	
n——*k*	*k*——*n'w'k*	*n*——	*k*——*-en'l*
	n——*n'w'k*		*n*——*-en'l*
k——*k*	*k*——*w'k*	*k*——*'l*	*w*——*w'l*
w——(*i*, *o*)	*w*——*w'l*	*w*——*'l*	*k*——*w'l*

When a noun begins with a vowel or with an *l* (really *'l* with inherent vowel), a dental is inserted after the pronominal prefix: *nt-akim,* 'my snow-shoe'; *nt-latwewâk'n,* 'my language.' When a noun begins with *w,* as *wíkwus,* the *w*-prefix coalesces with the initial, as shown above. Many substantives beginning with *m,* especially those denoting a part of the body, lose their *m* when inflected possessively: *m'huk,* 'body'; *n'huk,* 'my body,' etc.

Substantival Modifications.

The following seven noteworthy modifications of the substantive appear in Passamaquoddy:

1. The so-called obviative or accusative-ending of the third person occurs only when the animate noun stands in connection with a verb or possessive pronoun in the third person. There is no inanimate obviative. This accusative, which is peculiar to all the Algonquin dialects, is denoted in Passamaquoddy by *-l* in the singular, and in the plural often by the absence of any ending, or by *-i, -o.* The following instances will suffice to illustrate the application of this form: (*a*) *w'nimíal haásw'l,* 'he sees the horse,' but *k'nimía haás,* 'thou seest the horse'; (*b*) *w't-aásw'l,* 'his horse'; pl. *w't-aás,* 'his horses'; *wikwús'l,* 'his mother'; pl. *wikwus,* 'his mothers'; (*c*) to express a dative relation: *w'mílan haásw'l skitápyil,* 'he gives (him) the horse to the man,' but *k'mílen haás,* 'I give thee a horse'; cf. also in the participle: *nōtáchil,* 'those who hear him.' As to form, note the sing. obv. *skitápyil,* 'man'; *mûinyil,* 'bear,' but pl. *skitá'pihi; mûini; haáso; mûso,* 'moose'; *ma'tekwéss-o,* 'rabbits';

wá'sis, 'children'; *hamwes,* 'bees.' Note the double obv. ending in *ép'sîyil,* and also regularly *ép'sî,* 'trees,' from *ep's.* The obv. pl. is irregular in the demonstrative pronouns, which are inflected as follows: *wut,* 'this'; obv. sing. *wut'l,* and *wahat;* pl. an. *wut'k;* obv. *wutihi* (often *wahat*); pl. inan. *wut'l; yut,* 'this' (nearer); obv. sing. *yut'l,* and *yahat;* pl. an. *yukt,* obv. *yu'tihi* (often *yahat*); inan. pl. *yu't'l; na,* 'that' (indeclinable); *ni, nit,* 'that' (nearer); obv. sing. *ni't'l,* and *nihit;* pl. an. *nikt;* obv. *nihit* (rarely *ni'tihi*); pl. inan. *nilt'l.* The distinction between the singular and plural obviative is often not observed, even in nouns and verbs.

This case frequently appears as the subject of a sentence. *pechihalina w'skinosís'k w'nichálkw'l,* 'then comes the lads' uncle'; *mach'kowdîtit pesēsmowi nisumatíchihi,* 'as soon as their star-husbands have gone away.' Sometimes the obviative is omitted entirely: *w'néklan lámp'kwinóskwesís'k,* 'he leaves the water-sprites'; a fairly common phenomenon. The obviative frequently appears in verbs: *wítāpekamáspenihi,* 'he was a friend to them.'

There is no trace in Eastern Algonquin of the so-called sur-obviative or third personal accusative of the Cree and Ojibwe.

2. The locative-instrumental is expressed by *-k;* pl. *-ikúk,* which has the force of a number of English prepositions; viz., 'at, by, from, in, into, on, to,' according to the directive force of the verb with which it stands in construction: *Péssank,* 'at Bar Harbor'; *kowás'k,* 'by means of a log,' a common locution; *wéchi-notgát'wul-to w'tûn'k,* 'they crawl out of his mouth'; *móskesí'to paskán'wikúk,* 'he crawls out of the pits'; *wíkwām'k,* 'in' or 'into the house,' according to the sense of the verb; *kowásnok,* 'on a log'; *k'm'tkinans'n'k,* 'to our land,' etc. The locative *-k* is regularly inflected with the possessive suffixes as follows: *nt'm'hîk'nk,* 'on my axe'; *k't'm'hîk'n'k,* 'on thy axe'; *w't'm'hîk'nk,* 'on his axe'; *k't'm'hîk'n'n'k,* 'on our axe' (incl.); *nt'm'hîk'n'n'k,* 'on our axe' (excl.); *k't'm'hîk'n'w'k,* 'on your axe'; *w't'm'hîk'n'w'k,* 'on their axe.'

3. The vocative element *-tuk* as in *nitapé'tuk,* 'O my friends'; *wásistuk,* 'O my children,' used only with the plural, had originally a dubitative meaning = 'as many as there are.' This force is still existent in Cree and Ojibwe.

4. The possessive suffix *-m* contains the same demonstrative element as the *-m* of *neg'm*, 'he, she, it': *nt-aás'm*, *k't-aás'm*, 'my, thy horse,' etc.

5. The diminutive *-sis* is very common: *t'm'hîk'nsís*, 'little axe'; *pílskwesís*, 'girl' (*pil*, 'young' + *skwe*, 'woman' + *sis*), etc.

6. Here must be noted also the movable future *-ch*, which may be affixed indifferently to nouns or to verbs: *k'nimíol-ch*, 'I shall see you,' but *wikwām-ch nimi'ton*, 'I shall see the house.' There is another more vivid future expressed by *lî*, 'go,' preceding the verb: *k't'lî-nimíol*, 'I shall (am going to) see thee.'

7. Finally in this connection, similar to the *-ch* is the movable conditional particle *-p*, as *nt'liáp'n'p sámakwān'k skatúchi pískononōk*, 'we (excl.) should go upon the water, if there were no fog'; here with the verb, but *tahalo-p ke'kw-yali kwilwatakw*, 'as if they were seeking something' (*ke'kw*). Sometimes this appears doubled: *níl'p-lo nt-étum-níswinén'p*, 'I should take it along.'

So far as I am aware there is no interrogative state such as occurs in Ojibwe. As in all Algonquin dialects, the genitive relation is expressed often by means of simple apposition: *aut niméskw'k*, 'road (*aut*) of the spirits'; *ótenesís w'lî-p'maus'win'w'k*, 'a village (*otenesis*) of good people'; often, however, by the locative *-k*; *sipsís'k skwú't'k*, 'birds of (in) fire' (*skwu't'k*). Sometimes the genitive is indicated by the possessive relation: *w'skínōsis w'nichálkw'l*, 'the lad his uncle'.

As shown above, the dative is expressed by means of the verb and obviative substantive, if the verb is in the third person.

THE VERB.

The imperative is the simplest form of the verbal root, as is the case in most languages; thus: *kwaskw*, 'run thou'; *kwaskw'híkw*, 'run ye'; *kwaskwech*, 'let him run.'

The present tense is the main tense of the Passamaquoddy verb, as from it the future, conditional and past are formed by means of suffixes. In fact, it may be truly said that the present is the only real tense of the language. The present is often used for the past in vivid narrative. The following example of the present of the

intransitive stem *p'mauso-*, 'walk, live' (=*p'mî*=prolongation + *aus*, 'live'), will suffice to show the combination of this intransitive form with the pronominal elements: *np'maus*, 'I walk'; *k'p'maus*, 'thou walkest'; *w'p'maúso*, 'he walks'; 'is walking'; *k'p'maús-îp'n*, 'we (incl.) walk'; *np'maús-îp'n*, 'we (excl.) walk'; *k'p'maús-î'pa*, 'you walk'; *w'p'maús'w'k*, 'they walk.' Note also the participle *p'múiso*, 'he is in the act of walking'; *kwen-aúsit*, 'as long (*kwen*) as he lives,' or *m'sî-eli-p'maúsit*, 'all the time (*m'sî*) while (*eli-*) he lives.' Sometimes the singular of the intransitive verb ends in *-in: n'kwáskwin*, 'I run'; (*k'*)*kwáskwin*, 'thou runnest'; *w'kwáskwin*, 'he runs.'

The combination of the pronominal elements with the transitive verb is the most difficult feature of the language and is sufficiently illustrated by the following paradigms and scheme of prefixes and suffixes. Note that all forms marked in the following schedules with a single asterisk are indefinite; those with two asterisks show a dative force (cf. **We—Thee,** Incl. in the Paradigm); forms with three asterisks have both indefinite and definite force, while all forms not designated are definite. All forms in Italics are negative and must be preceded by the negative particles *kat, katama, skat,* 'not.' All forms in Roman are positive. The *-ep, -p, -s, -epus* forms in parentheses indicate the imperfect or conditional-subjunctive. **Ind.** = Indefinite; **Def.** = Definite; **P:** = the Imperfect (Past); **An.** = Animate; **Inan.** = Inanimate. All forms in Roman are positive.

PARADIGM OF NIM—"TO SEE."

I—Thee: k'nimíol(-ep); *k'nimiólo(-p; -pus)*. When the verb-stem ends in -l, as mil, 'give,' this form becomes k'milen.

I—Him-Her:[3]nimía (-p); *nimiáwi (-p; -s)*.

I—It: Ind. nimi'to (-p'n); *nimi'towi (-p; s)*. **Def.** nimi'ton(-ep); *nimi'towun (-ep)*.

I—You: k'nimiólpa (-p); *k'nimioló'pa(-pus)*.

I—Them (An.): Ind. nimía; *nimiawíwu (-p)*. **Def.** nimíuk; **P.** nimiápenik; *nimiawíwuk;* **P:** *nimiawiwápenik.*

[3] In this verb, the forms: *nimía, nimi'to, nimi'ton'l* stand for *n'nimía, n'nimi'to,* etc., with the prefix *n'* of the first person, assimilated to the *n* of the verbal root. The second personal *k'* also assimilates to a *k*-stem: *kwáskwin,* 'thou runnest.'

I—Them (Inan.): nimí'ton'l; **P:** nimi'tonépenil; *nimí'towunul;* **P:** *nimí'towunépenil.*

Thou—Me:[4] k'nimí-h-i (-p; -pus); *k'nimihíwi(-p).*
Thou—Him-Her: k'nimía(-p); *k'nimiáwi(-p).*
Thou—It: Ind. k'nimíto(-p); *k'nimí'towi(-p).* **Def.** k'nimíton-(-ep); *k'nimí'towun(-ep).*
Thou—Us: k'nimiáp'n(-ep; -es); *k'nimiawíp'n (-ep).*
Thou—Them (An.): Ind. k'nimía; *k'nimiawíwu.* **Def.** k'nimíuk; *k'nimiawíwuk;* **P:** k'nimiápenik; *k'nimiawiwápenik.*
Thou—Them (Inan.): Ind. k'nimi'tónia; *k'nimi'towuno.* **Def.** k'nimi'ton'l; *k'nimí'towun'l.*

He-She—Me: nimióg'n(-ep); *nimióg'wi(-p).*
He-She—Thee: k'nimióg'n(-ep); *knimióg'wi(-p).*
He-She—Him-Her: Ind. w'nimía(n);[5] *w'nimiáwi (-p'n).* **Def.** w'nimíal; **P:** w'nimiáp'n; *w'nimiawíwul;* **P:** *w'nimiawíp'n.*
He-She—It: Ind w'nimi'to (-p) *w'nimi'towi(-p).* **Def.** w'nimi'-ton(-ep); *w'nimi'towun(-ep).*
He-She—Us: k'nimióg'nen(-ep); *k'nimiog'wínen(ep).*
He-She—You: k'nimióg'wu (-ep); *k'nimiog'wíwu (-p).*
He-She—Them (An.): Indef. w'nimía; *w'nimiawíwu.* **Def.** w'nimíuk; **P:** w'nimiápenik; *w'nimiawíwuk;* **P:** *w'nimiawiwápenik.*
He-She—Them (Inan.): w'nimí'ton'l; **P:** w'nimi'tonép'n; *w'nimi'tówunul;* **P:** *w'nimi'towunépenil.*

We (Incl.)—Thee: k'nimiólp'n (-opus); *k'nimiolóp'n;* **P:** *k'nimiolopóp'n (-us),* but note k'pechíptolnén(-ep) = **Def.** 'we bring it to thee'; *k'pechiptolónen(-ep).*
We (Incl.)—Him-Her: Indef. k'nimiáp'n (-ep): *k'nimiawíp'n-(-ep).* **Def.** k'nimiánen (-ep); *k'nimiawínen (-ep);* k'nimianna; *k'nimiawinna.*
We (Incl.)—It: Indef. k'nimi'tonép'n(-ep); *k'nimi'towíp'n (-ep).* **Def.** k'nimí'tonén (-ep); *k'nimí'towinén (-ep).*

[4] The element *-h-* is a connecting consonant here.
[5] The *n*-form, really participial, is often definite; see note 9 below.

We (Incl.)—You:[6] k'nimiólp'n (-ep and -opus); *k'nimiolóp'n;* **P:** *k'nimiolopóp'n.* This form has a similar dative combination to that in **We (Incl.)—Thee** above, i. e., k'—olnen; *k'—olonen.*

We (Incl.)—Them (An.): Indef. and **Def.**[7] k'nimiánnawa; **P:** k'nimiannawápenik; *k'nimiawínnawa;* **P:** *k'nimiawinnawápenik.* **Def.** k'nimiánnawuk; **P:** k'nimiannawépenik; *k'nimiawínnawuk;* **P:** *k'nimiawinnawépenik.*

We (Incl.)—Them (Inan.): k'nimi'tonénwul; **P:** k'nimi'tonenépenil; *k'nimi'towinén'wul;* **P:** *k'nimi'towinenépenil.*

We (Excl.)—Thee, Him-Her, etc., differs from the above forms only by the n-preformative instead of the k'.

You—Me:[4] k'nimi-h-í'pa; *k'nimi-h-iwí'pa.*

You—Him-Her: Indef. k'nimiá'pa (-p); *k'nimiawí'pa* (*-p*). **Def.** k'nimiáwa (-p'n); *k'nimiawíwa*(*-pn*).

You—It (Indef.): k'nimitó'pa(-p'n); *k'nimitowí'pa*(*-p'n*). **Def.** k'nimitónia; **P:** k'nimitoniáspenil; *k'nimi'towúno;* **P:** *k'nimi'towiwáspenil.*

You—Us:[4] k'nimi-h-íp'n (-ep; epus); *k'nimiyawíp'n*(*-ep; epus*).

You—Them (An.): Indef. k'nimiánia; *k'nimiawiyania.* **Def.** k'nimiáwa; **P:** k'nimiawépenik;[7] *k'nimiawíwu;* **P:** *k'nimiawiwépenik.*

You—Them (Inan.): Indef. k'nimi'tónia; *k'nimi'towúno;* **P:** k'nimi'tonépenil; *k'nimi'ton'wiwépenil.* **Def.** k'nimi'ton'wul; *k'nimi'towún'wul.*

They—Me: nimióguk; **P:** nimiogópenik;[8] *nimiog'wíwuk;* **P:** *nimiog'wiwépenik.*

They—Thee: k'nimióguk; **P:** k'nimiogópenik; *k'nimiog'wíwuk;* **P:** *k'nimiog'wiwépenik.*

[6] Note that **We-Thee** is identical with **We-You,** no distinction being made here between the singular and plural object. But cf. **They-Thee** and **They-You,** where the distinction is made!

[7] Note the apparently arbitrary difference in vowel in the past: *-wápenik* for the **Indef.** and *-wépenik* for the **Def.** Observe that *k'nimiánnawa* is used both with indefinite and definite nouns. When the noun is not expressed *k'nimiánnawuk* is used.

[8] The *o* in *-ópenik* is plainly due to vowel harmony from the *ŏ* inherent in *nimiŏguk,* but note the *ŏ* in *w'nimi'towiwŏpenik,* 'they do not see it,' where the *o*-vowel seems to be due to assimilation to the negative *w.*

They—Him-Her: Indef. w'nimiánia; **P:** w'nimiápenik; *w'nimiawiyánia;* **P:** *w'nimiawiwápenik.* **Def.** w'nimiáwul; **P:** w'nimiawápenik; *w'nimiawíwul;* **P:** *w'nimiawiwápenik.*

They—It: Indef. w'nimí'towuk; **P:** w'nimi'tópenik; *w'nimi'towíwuk;* **P:** *w'nimi'towiwópenik;*[8] **Def.** w'nimi'tónia; **P:** w'nimi'toniápenil; *w'nimi'towúno;* **P:** *w'nimi'towuniápenil.*

They—Us: k'nimióg'n'wuk; **P:** k'nimióg'nópenik; *k'nimiog'win'wuk; k'nimiog'wiwápenik.*

They—You: k'nimióg'wuk; **P:** k'nimiogópenik; *k'nimiogowíwuk;* **P:** *k'nimiogowiwépenik.*

They—Them (An.): Indef. w'nimiánia; **P:** w'nimiawápenik; *w'nimiawawiyánia;* **P:** *w'nimiawiwápenik.* **Def.** w'nimiáwa or -wul; **P:** w'nimiawépenik; *w'nimiawíwu* or *-wul;* **P:** *w'nimiawiwápenik.*

They—Them (Inan.): Indef. w'nimi'tónia; **P:** w'nimi'toniápenil (-aspenil); *w'nimi'towúno;* **P:** *w'nimi'towuniápenil* (*-aspenil*). **Def.** w'nimi'tón'wul; *w'nimi'towún'wul;* **P:** w'nimi'tonwépenil; *w'nimi'towunépenil.*

Participial Forms.*

I—Thee: nimíolún; táyowe k'nimíol; *nimiólowun; tayowe k'nimiólo.*

I—Him-Her: nimían; táyowe nimía; *nimiawíwun; táyowe nimiáwi.*

I—It: nimí'toan; táyowe nimí'to(n);[9] *nimi'towíwun; tayowe nimi'towi.*

[9] The *n*-form seems to be optional.

I—You: táyowe k'nimólpa; *táyowe k'nimioló'pa.*

I—Them (An.): táyowe nimíuk; *táyowe nimiawíwuk.*

I—Them (Inan.): nimí'toanul; *nimi'towíwanul.*

Thou—Me: nimihíyin; *nimihiwíyin.*

* These forms are used in subordinate as well as in hanging clauses; thus, *nimíolun,* '(when) I see thee,' is also expressed by the finite construction, *táyowe k'nimíol.* In many instances the purely participial form seems to be lacking, as in **I-You, I-Them,** etc. I believe, however, that there are participial forms for all the combinations; possibly forms which my Indian informant did not happen to think of at the moment. In the kindred Canadian Abenaki, it is possible to place purely participial forms for all almost all the possible phrases (Prince, *Miscellanea Linguistica in Onore di Graziodio Ascoli,* p. 358, Ascoli Memorial).

Thou—Him-Her: k'nimíyas;[10] nimían; *nimiawíyan.*
Thou—It: k'nimitoánes;[11] nimi'toan; *nimi'towíwun.*
Thou—Us: k'nimiyápenus;[11] nimiyáp'n; *nimiawíp'n.*
Thou—Them (An.): k'nimíuk; nimían(?);[9] *nimiwíyan.*
Thou—Them (Inan.): nimi'toanul; *nimi'towiwánul.*

He-She—Me: nimíhit; *nimihikw.*
He-She—Thee: k'nimíog:[12] *k'nimióg'wi.*
He-She—Him-Her: táyowe w'nimíal; nimiá'tit; *nimiyakw.*
He-She—It: nimi'toan; *nimi'towiwun.*
He-She—Us: táyowe k'nimióg'nen; *k'nimiog'winen.*
He-She—You: táyowe k'nimióg'wu; *k'nimiog'wiwu.*
He-She—Them (An.): w'nimiá'tit;[13] *w'nimia'tíkw(?).*
He-She—Them (Inan.): táyowe w'nimí'towul; *w'nimi'towiwul.*

We—Thee: táyowe k'nimiólp'n; *táyowe k'nimiolóp'n* (*-ópus*).
We—Him: táyowe k'nimiánen or k'nimiáp'n; *táyowe k'nimiawíp'n; nimiyáwin* (*-us*).
We—It: táyowe k'nimí'tonen; *táyowe k'nimi'towínen; nimi'tow'néwin* (*-us*).
We—You: táyowe k'nimiólp'n; *táyowe k'nimiolóp'n*(*opus*).
We—Them (An.): táyowe k'nimiánnawuk; *táyowe nimiáwan* (*-óspenik*).
We—Them (Inan.): táyowe k'nimi'tonénwul; *táyowe nimi'tow'néwin* (*-óspenil*).

You—Him-Her: táyowe kílwau k'nimiyáwa; *táyowe k'nimiyawíwa.*
You—Him-Her: táyowe kílwau k'nimiyáwa; *táyowe k'nimiyáwiwa.*

[10] The ending *-s* is common in the past (cf. **We-Them,** participle) and may often be substituted for the characteristic *-p*, or even combined with it, as *-pus* (**We-Thee; We-You**). I suspect that *k'nimíyas, k'nimi'toánes, k'nimiyápenus* are really past forms here; i. e., that my informant understood the English 'see' in the past sense, following the New England dialect of English where 'see' = 'saw.' The *nimían*-form, which is purely participial, seems to be indeterminate, as it means 'I seeing him,' 'thou seeing him, them.' This is also the case with Abenaki *namihoan, namitoan.*

[11] Finite forms; *nimi'toan* and *nimiyáp'n* are the real participles.

[12] Note absence of the finite *-n*; *k'nimiōg'n,* 'he sees thee.'

[13] It is strange to find the *w*-prefix before a participial form; cf. **They-Them** in this list.

You—It: táyowe k'nimi'tónia; *tayowe k'nimi'ton (-iaspenil)*;[14] *sic!*
You—Us: tayowe k'nimihíp'n; *tayowe k'nimiawíp'n.*
You—Them: nimiówuk; *nimiowíwuk.*
You—Them (Inan.): táyowe k'nimi'tónia; *táyowe k'nimi'towúnwul.*

They—Me: nimihiyóguk;[15] *nimihiog'wíwuk.*
They—Thee: táyowe k'nimióguk: *táyowe k'nimiog'wíwuk.*
They—Him: táyowe w'nimiáwul; *táyowe w'nimiawíwul.*
They—It: táyowe w'nimi'tónia; *táyowe w'nimi'towúno.*
They—Us: táyowe k'nimióg'n'wuk; *táyowe k'nimiog'n'wín'wuk.*
They—You: táyowe k'nimióg'wuk; *tayowe k'nimiogowíwuk.*
They—Them (An.): táyowe w'nimia'títit[13] or nimiá'tit; *táyowe w'nimiawíwu.*
They—Them (Inan.): táyowe w'nimia'tótit; *tayowe w'nimi'towún'wul.*

PASSIVE.

'I am seen,' nimióguk (='they see me'); (*katama*) *nimiog'wíwuk.*
'Thou art seen,' k'nimíuk;[16] (*katama*) *k'nimiokiū.*
'He is seen,' w'n'míkw'so; (*katama*) *w'nimikw'siū.*
'We are seen,' k'nimiokép'n; (*katama*) *k'nimiokép'n.*[17]
'You are seen,' k'nimioképa; (*katama*) *k'nimioképa.*
'They are seen,' nimíkw's'wuk; (*katama*) *nimikw'siwíyik.*

'TO HAVE' (AN. OBJECT SING.).

'I have,' nt-í-wa (-p);[18] *nt-i-yiwáwiw (-wip'n).*
'Thou hast,' k't-í-wa (-p); *k't-iyiwáwiw (-wip'n).*
'He has,' w't-í-wul; **P:** w't-i-wap'n or w't-wáspenil; *w't-í-wawíwul;* **P:** *w't-i-wawíp'n;* but w't-í-yowan, 'he has them.'
'We (Incl.) have,' k't-i-wáp'n (-ep); *k't-i-yiwawíp'n (-ep).*
'You have,' k't-i-wá (-pn); *k't-i-yiwáwina (-p'n).*

[14] This can hardly be correct as a negative form. It seems to be a past form, owing to the *-s-* in *-áspenil.*

[15] Apparently a pure participial formation. The finite form 'they see me' = *nimiōguk.*

[16] Apparently a pure passive. Note the finite *k'nimióguk,* 'they see thee.'

[17] It is strange to find no distinctively negative form here.

[18] Probably should be *nt-i-wap'n, kt-i-wap'n* in the past.

'They have,' **Indef.** w't-i-yánia; **P:** w't-i-yápenik; *w't-i-yiyawiyánia.*
Def. w't-i-yáwul; **P:** w't-iyawáp'n; *w't-iyawíwul.*

'TO HAVE' (INAN. OBJECT ALSO = 'TO BE').

n't-í-yin (-es; -ep'n).
k't-í-yin (-es; -ep'n).
w't-í-yin (-es; -ep'n).
k't-i-yíp'n (-es; -ep'n).
k't-i-yí'pa (-es; -ep'n).
w't-i-yínia; **P:** w't-i-yipenik.
w't-í-wul; **P:** w't-i-yipenil.

IRREGULAR ELEMENTS OF "TO HAVE" AND "TO BE."

Third Person Singular: al-ech, 'let it be'; el-e-sin,[19] 'where he is'; eyik, 'he (really 'it') is'; eyit, 'where he is'; el-eyit, 'where he is'; el-i-y-ijil = obviative of eyit; eyin, 'it being'; meskw nit el-i-nook, 'before this is so' (neg.);[20] tan ot'l-i-yin,[21] 'however it may be'; ch'wi-l-eyo, 'it must be'; w't-ach'wi-t-iwal, 'he must have it'; kis-iyit,[22] 'it having been'; w'-kichiyawi-wanyogonia, 'they have enough of it.'

Third Person Plural: m'si ayale, 'all who are'; eyoltitit, 'they being' (reflex.) eli-y-oltitit, 'as they are' (reflex.); el-igek, 'they who are'; etutek, 'they being'; eyilit; obv. eyilijil, 'where they are'; iakw, 'where they are'; weji-ya-witits, 'where they were' (with -s = Past).

SCHEME OF VERBAL PREFIXES AND SUFFIXES.

	ME.	THEE.	HIM-HER.	IT.
I		k—ol, -el (-en)	n—a, u	* n—to
		k—olo	*n—awi*	* *n—towi*
				n—ton
				n—towun

[19] Note *e* for *i* in *el-e-sin, ēyik, ēyit,* due to vowel harmony. 'he has taught me.'

[20] *Meskw,* which is used in the sense 'before,' really means 'not yet,' and therefore takes the negative verb.

[21] Note the phonetic *l* before the vowel root and the connecting *-t-* between the prefix and the *l.*

[22] *Kis,* 'already,' is the sign of the perfect tense; thus, *nkis-ake'kimkon,* 'he taught me.'

Thou	k—i *k—iwi*		k—a, u *k—awi*	* k—to * *k—towi* k—ton *k—towun*
He-She	n—gon, g'n *n—g'wi*	k—gon, g'n *k—g'wi*	*w'—a(n) **w—awi* w—al *w—awiwul*	*w—to **w—towi* w—ton *w—towun*
We (Incl.)		*k—olp'n **k—olop'n* **k—olnen *k—olonen*	* k—ap'n * *k—awip'n* ** k—anen, anna *k—awinen, awinna*	* k—tonep'n * *k—towip'n* k—tonen *k—towinen*
We (Excl.)		*n—olp'n **n—olop'n* **n—olnen *n—olonen*	* n—ap'n * *n—awip'n* n—anen, anna *n—awinen, awinna*	* n—tonep'n * *n—towip'n* n—tonen *n—towinen*
You	k—i'pa *k—iwi'pa*		* k—a'pa * *k—awi'pa* k—awa, u *k—awiwa(u)*	* k—to'pa * *k—towi'pa* k—tonia *k—towuno*
They	n—oguk *n—og'wiwuk*	k—oguk *k—og'wiwuk*	*w—ania **w—awiyania* w—wul *w—awiwul*	*w—towuk **w—towiwuk* w—tonia *w—towuno*
	Us.	You.	Them (An.).	Them (Inan.).
I		k—olpa *k—olo'pa*	* n—a, u * *n—awiwu* n—uk *n—awiwuk*	*** n—ton'l *n—towun'l*
Thou	k—ap'n *k—awip'n*		* k—a, u * *k—awiwu* k—uk *k—awiwuk*	* k—tonia * *k—towuno* k—ton'l *k—towun'l*
He-She	k—og'nen *k—og'winen*	k—og'wu *k—og'wiwu*	*w—a, u **w—awiwu* w—uk *w—awiwuk*	***w—ton'l *w—towun'l*

We (Incl.)		k—olp'n *k—olop'n* **k—olnen ***k—olonen*	* k—annawa * *k—awinnawa* k—annawuk *k—awinnawuk*	*** k—tonen'wul *k—towinen'wul*
We (Excl.)		*n—olp'n **n—olop'n* **n—olnen ***n—olonen*	* n—annawa * *n—awinnawa* n—annawuk *n—awinnawuk*	*** n—tonen'wul *n—towinen'wul*
You	k—ip'n *k—awip'n*		* k—ania or * k—awa *k—awiwiyania* k—awu *k—awiwu*	* k—tonia * *k—towuno* k—ton'wul *k—ton'wun'wul*
They	k—og'n'wuk *k—og'win'wuk*	k—og'wuk *k—og'wiwuk*	*w—ania **w—awiyania* w—awa, u *w—awiwul*	*w—tonia **w—towuno* w—ton'wul *w—towun'wul*

A study of the above forms will indicate that the main points of difference between the indefinite and definite combinations lie in the third person singular and throughout the plural. Although, as pointed out above, there is really only one tense, the present, there are certain endings which may be affixed to denote the past relation and the conditional-subjunctive. Thus, the *-p, -ep, p'n* and *-s*-elements are used for the past and conditional-subjunctive alike, and even appear in combination together, as *p's; ep's; op's.* A careful distinction must be made between the past-conditional *-p'n* and the *-p'n* of the first person plural as seen in **Thou—Us** and also in the intransitive verb as given above.

There is also, as shown, a passive voice, the distinctive element of which is *-k, -s* and in combination *-kw's.* Note also the passive forms *m'skówa,* 'he will be found'; *w'metapéksin,* 'it is finished'; *milkónia wásís'l,* 'they are given to the child'; *weswéphogónia,* 'that they be taken back'; *nōtakw'siánp'n,* 'I was heard' (*nōt-*); *kitwitasso,* 'it is called.' Sometimes a reflexive is used for a passive: *elokélit,* 'what was done.' The reflexive, not indicated in the paradigms, is expressed in various ways; *e. g.,* by an *l-,* insert: *w'm'tyayew'lít-el-ín,* 'as if they were playing together'; *kinw-el-úswiū,* 'it shows itself'; *nt'lî-kisi-kwálp-el-es,* 'I will change myself,; *mache-*

kaú-d-íl-it, 'as soon as they have gone off together,' etc. Motion is expressed by *pech-: 'pechiyan,* 'when I come'; *w'p'chitakan,* 'he sends him'; *pechi-pawatmat,* 'he is always desirous'; *pechipha,* 'he brings him,' etc.

'Must' is expressed by the insert *ach'wi: w't-ách'wi-sakitón'l,* 'he must rule'; and in the future: *k't-ách'wi-t'lî-wíchiy'n,* 'thou shalt be (*t'li-*) compelled to take heart.'

Desire is indicated by *ktî-: w'k'tî-nimíal,* 'he wishes to see him'; *k'tî-elóʻkelít,* 'what he wishes to do.'

There are many other such particles too numerous to mention in an article of this length.

As in all other Algonquin idioms, in the combined forms, the second person always takes precedence over the first and the first person over the third. Thus, in the forms *k'nimíol,* 'I see thee,' where the second person is the object and in *k'nimiáp'n,* 'you see me,' where the second person is the subject, the second personal element comes first. In such forms, however, as *nimía,* 'I see him'; *nimiʻto,* 'I see it,' the first person appears in the first place.

The sign of the negative is the infixed *u*-vowel which, as shown above, frequently appears as *o* and often as *w*.

The use of the participle is most varied. Thus, it may take the place of the relative form as *nit pawálkwak,* 'this is what is wanted' (passive indicated by *-kw*), or it may be used to denote the action of the verb governed by a preposition *wechi-nimíolun,* 'in order for me to see thee,' or else it may be employed as a conditional: 'if' or 'when I see thee' = *nimíolun.* The negation of the participle is formed in the same way as the negative of the finite forms, *viz.,* by infixation of *u, o, w.*

Any noun may be verbalized by the ending *e-: w'skitáp-e,* 'he is a man'; by *-ewi: tan etúchi w'skitápewi,* 'so long as he is a man'; by *-ewiū: w'skitápewíū,* 'he becomes a man'; also by participial endings: *w'skitápewít,* 'he who is a man'; or by *-(w)eleso: w'skitápewéleso,* 'he becomes a man.'

Practically all the Passamaquoddy verbs are conjugated after the above model, most of the minor variations which occur being due to phonetic peculiarities.

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NUMERALS.

The numerals up to five present three forms; *viz.*, a form used chiefly in counting, and adjectival animate and inanimate forms, as indicated by the following list. There is no trace of peculiar numerals used only with certain classes of substantive, as, for example, with round objects, etc., such as occur in Ojibwe.

	In Counting,	Animates.	Inanimates.
one	*nekw't*	*peskw;* obv. *-'l* *peskw* 'one, each' occasionally: *nekw't*	*peskw* occasionally: *nekw't*
two	*tábū*	*nĭswuk* obv.: *nĭsō*	*nĭsn'l*
three	*sist*	*nṓwuk* obv.: *nōhō*	*nṓw'n'l*
four	*nḗu*	*nḗwuk* obv.: *nēwō*	*nḗw'n'l*
five	*nā́n*	*nā́n'w'k* obv.: *nānō*	*nā́n'w'l*
six	*kamáchin*	*kamáchin-keswuk* obv. *-kesō*	*kamáchin-kesn'l*
seven	*'lwĭk'n'k*	*'lwĭk'n'k-keswuk* obv. *-kesō*	*'lwĭk'n'k-kesn'l*
eight	*'km"lchin*	*'km"lchin-keswuk* obv. *-kesō*	*'k"mlchin-kesn'l*
nine	*eskw'nátek*	*eskw'nátek-keswuk* obv. *-kesō*	*eskw'nátek-kesn'l*
ten	*m'tul'n*	*m'tul'n-keswuk* obv. *-kesō*	*m'tuln'-kesn'l*

From eleven to fifteen, the numerals are formed from the tens by affixing for the animate and inanimate *-ankoso-wuk;* obv. *ankoso; ankosow'l,* respectively: *nisánkosowuk skitayik,* 'twelve men'; *nsánkoso-wuk* (obv. *-wo*); inan. *-w'l,* 'thirteen'; *newánko-,* 'fourteen,; *nananko,* 'fifteen.' From sixteen to nineteen the affixed element is *-kesánko: kamáchin-kesánkosowuk* (obv. *-kesánkoso*); inan. *-kesánkosow'l,* etc.

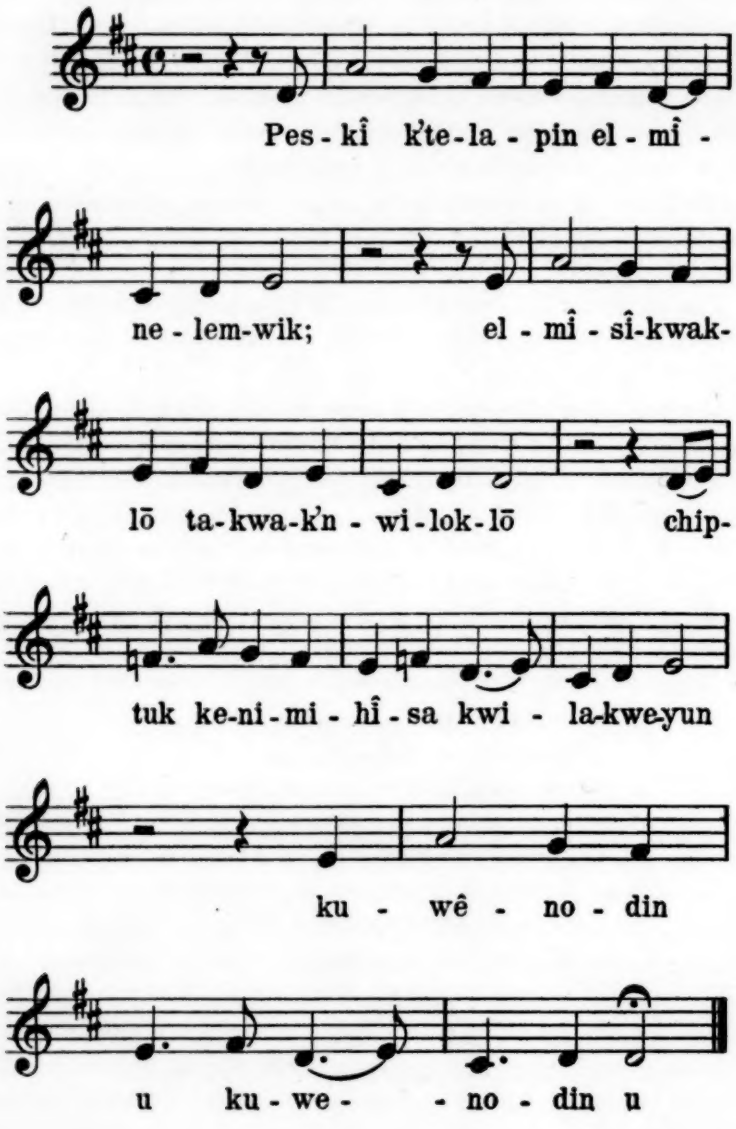
From twenty on, the cardinal elements are as follows: *nisínsk-,* 'twenty'; *nsínsk-,* 'thirty'; *nēowinsk-,* 'forty'; *nanínsk-,* 'fifty'; *kamáchin-kesínsk-,* 'sixty'; *'lwĭk'n-kesínsk,* 'seventy'; *'km"lchin-kesínsk,* 'eighty'; *eskw'nátek-kesínsk,* 'ninety'; *nekw'tát'kw-,* 'hun-

dred.' The animate and inanimate plurals are made from these forms by affixing the element; animate *-ke'kw'sowuk* (obv. *-ke-'kw'so*); *inan. -ke'kw'-sow'l;* as *nēowínsk-ke'kw'sowuk skitápyik,* 'forty men,' etc.

The ordinals, with the exception of *amsk'was,* 'first,' are formed from the cardinals by adding *-ewéi* (*nisewéi,* 'second'; *nōwewéi,* 'third,' etc.), until tenth which is *nekw'tinskewéi.* To the element *-anko* of the -teens is added the ending *-wewei; nēowánkowewéi,* 'fourteenth,' and to the ending *-insk* is added *-ekewei;* as *nisinske-kewéi,* 'twentieth.'

The numerals are usually inflected adjectivally preceding their substantives, but they may be used indeclinably, as *eskw'nátek-kesōk'niū,* 'nine days.'

The following love-song will serve to illustrate both the present musical style of the tribe, which is undoubtedly influenced by the Roman Catholic Gregorian chants of their missionary priests, and also the construction of the language. It should be noted that the last syllable *û* of the song must be prolonged as much as possible, and finally allowed to end with a rapid expulsion of the breath, this is the so-called "die-away" which is a characteristic of much of the American music.

Peskî k't-el-ápin elmî-nelemwík
Lonely thou lookest up-stream
Elmî-sîkwak-lo takwâk'nwi-lok-lo
In spring and in autumn;
Chîptuk k'nimihí-sa kwilakweyún
Perhaps thou mayest see me seeking thee.
Kuwēnodîn Û; kuwēnodin Û.
It is long O; It is long O!

Pes - kî k'te-la - pin el - mî -
ne - lem-wik; el - mî - sî-kwak-
lō ta-kwa-k'n - wi-lok-lō chip-
tuk ke-ni-mi - hî - sa kwi - la-kwe-yun
ku - wê - no - din
u ku - we - - no - din u

COMMENTARY.

In the above song, *peskî* is adverbial from *peskw*, 'one; lonely'; *k't-el-apin: k* = second person + the infixed *-t-* before a vowel + *el*, the element of prolongation, 'thou art doing it' + *ap*, 'look' + the intransitive *in*, seen above in the conjugation of *kwaskw*, 'run'; *elmî-nelemwík: elmî* = 'being; while there' + *nelemwík*, which also occurs in the form *nul'muk*, showing the indeterminate vowel = 'up' —here probably 'up-stream'; *elmî-sikwak-lo: elmî*, here, = 'during' + *sîkwak*, a musical prolongation of *sîkw'k*, 'in spring' + *lō*, the asseverative particle; *takwâk'nwi-lok-lo: takwâk'n*, 'autumn,' + the adjectival *-wi* + the verbal inan. *-lok* + the asseverative *-lo; chîp-tuk*, probably contains the same element as the future *-ch* + the verbal *p't* + the inan. participle *-uk* = 'when it may be'; *k'nimi-hí-sa*, 'thou seest me' with the conditional *-sa; kwilakwiyun: kwil*, 'seek,' + the formative *ak* + *(w)iyun*, participial, = **I — Thee;** *kuwēnodin U: kwēn*, 'be long,' + the intransive endings *-(o)din* + the exclamation *U* = 'Oh.'

DISCUSSION OF "A KINETIC THEORY OF GRAVITATION."

(PLATE I.)

BY CHARLES F. BRUSH.

(*Read April 24, 1914.*)

1. GRAVITATION IS DUE TO INTRINSIC ENERGY OF THE ETHER.

At the Minneapolis meeting of the American Association for the Advancement of Science I had the honor to outline "A Kinetic Theory of Gravitation,"[1] which is in substance briefly as follows:

The ether is assumed to be endowed with vast intrinsic kinetic energy in wave form of some sort capable of motive action on particles, atoms or molecules of matter, and propagated in every conceivable direction so that the wave energy is isotropic. The waves are of such low frequency, or otherwise of such character, that they pass through all bodies without obstruction other than that concerned in gravitation. Distribution of the ether's energy is uniform throughout the universe except as modified by the presence of matter.

Atoms or particles are imagined to be continually buffeted in all directions by the ether waves like particles of a precipitate suspended in turbulent water. There are no collisions because neighboring particles follow very nearly parallel paths.

Each particle or atom of matter is regarded as a center of activity due to its energy of translation initially derived from the ether. There is continual absorption and restitution of the ether's energy, normally equal in amount; but the ether is permanently robbed of as much of its energy as is represented by the mean kinetic energy of the particle or atom. The particle or atom thus has a field of influence extending in all directions, or casts a spherical energy shadow, so to speak, the depth or density of the shadow varying with the inverse square of distance. The energy shadow of a body of matter

[1] *Science*, March 10, 1911; *Nature*, March 23, 1911.

is the sum of the shadows of its constituent parts. The energy shadows of two gravitating bodies interblend, so that the energy density between them is less than elsewhere, and they are pushed toward each other by the superior energy density, or wave pressure, on the sides turned away from each other.

That the ether really *is* endowed with vast intrinsic energy in some form or forms is the belief of many eminent physicists, and it seems to me highly probable that *all* energy has its source and destination in the ether; that is to say, that energy in all the various forms in which we observe it, comes in some way from the ether and is energy *of* the ether. This view does not in any manner conflict with the principle of conservation of energy.

In support of my contention that etherial energy is the cause and essence of gravitation, I wish to emphasize particularly, what seems to me an obvious fact, that the energy acquired by a falling body comes from the ether, and is restored to the ether when that body undergoes negative gravitational acceleration.

In this connection I cannot do better than quote Lord Kelvin's description of the collision of two very large bodies through the influence of gravitation. In his "Popular Lectures and Addresses" (Vol. 1, 413–417) he says:

"To fix the ideas think of two cool solid globes, each of the same mean density as the earth and half the sun's diameter, given at rest, or nearly at rest, at a distance asunder equal to twice the earth's distance from the sun. They will fall together and collide in exactly half a year. The collision will last for about half an hour, in the course of which they will be transformed into a violently agitated incandescent fluid mass flying outward from the line of motion before the collision and swelling to a bulk several times greater than the sum of the original bulks of the two globes. . . . The time of flying out would probably be less than half a year when the fluid mass must begin to fall in again towards the axis. In something less than a year after the first collision the fluid will again be in a state of maximum crowding towards the center, and this time even more violently agitated than it was immediately after the first collision; and it will again fly outward, but this time axially towards the places whence the two globes fell. It will again fall inwards, and

after a rapidly subsiding series of quicker and quicker oscillations it will subside, probably in the course of two or three years, into a globular star of about the same mass, heat and brightness as our present sun."

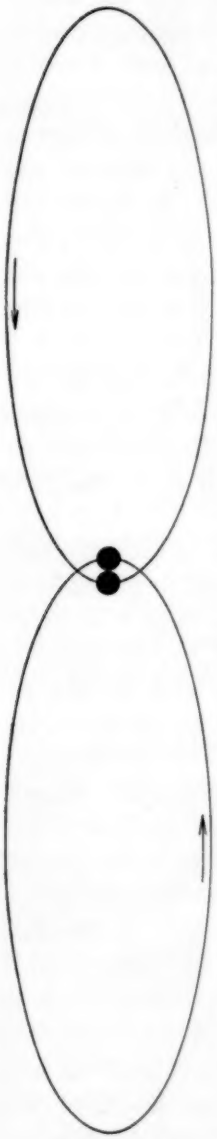


FIG. 1.

Undoubtedly this is a substantially correct description of what would happen under the conditions named. The two cold bodies would acquire *from some source external to themselves* the vast energy represented by the heat of the sun, heat sufficient to maintain the enormous solar radiation millions of years without sensible diminution. And this vast accumulation of energy would occur in half a year, largely in the *last few days* before collision. There is, to me, no conceivable source of this energy other than the ether. It may be argued that the two cold bodies, as a gravitating system, initially possessed all this energy in the form of "potential energy of position." This is a most convenient expression, but it affords no explanation of the *source* of the energy until, as I pointed out at the Washington meeting, we take the energy-endowed ether into partnership as an essential part of the system. Certainly the energy could not be resident in the two cold motionless globes. For a homely illustration, think of two golf balls joined by a stretched thread of rubber; they form an attracting system and possess "potential energy of position" or separation, but the energy does not reside in the balls, it is in the stretched rubber thread.

Later in his description Lord Kelvin says: "If, instead of being at rest initially, . . . each globe had a transverse velocity of three quarters (or anything more than .71) of a kilometer per second, they would just escape collision, and would revolve in ellipses

round their common center of inertia in a period of one year, just grazing each other's surface every time they came to the nearest points of their orbits." (Assuming of course that the globes were sufficiently rigid to escape disruption by tidal forces.)

To aid in forming a mental picture of this last case described by Lord Kelvin, in which the two globes fall together but do not collide, I have made a diagram (Fig. 1) of the two elliptical orbits; and in order to show the globes of appreciable size, the orbits are made very much less excentric than Kelvin's premises call for. The globes are shown at perihelion, just escaping collision. Of course, the globes in falling from aphelion to perihelion would gather the same amount of energy that they did in the case of collision, where their motion was arrested and their kinetic energy was thus converted into heat; but without collision the vast energy acquired during positive acceleration from aphelion to perihelion would disappear during negative acceleration from perihelion to aphelion, and be transformed back to the ether whence it came.

The sun and planets of the solar system, and the planets and their satellites, because of the excentricity of their orbits, continually go through the same kind of cycle described by Lord Kelvin, differing from that only in degree. For instance, the earth in its six months' passage from aphelion to perihelion falls about three million miles toward the sun, and gains in orbital velocity about five eighths of a mile per second. It thus acquires new kinetic energy from the ether which, if it could be manifested as heat, would be sufficient to evaporate all the oceans, lakes, and rivers, heat the dry earth to vivid incandescence, and vaporize much of it; the earth would become a miniature sun. And all this energy is restored to the ether during the next half year while the earth is moving from perihelion to aphelion.

With the idea in mind that a falling body gathers energy from the ether, and restores all of it to the ether when raised the same distance against gravitation, *by any means,* homely examples are at once suggested; thus, a stone thrown upward and falling again, does it in the reverse order, and a common clock pendulum goes through, and repeats the cycle with almost the regularity of a sun and planet.

In the theory of gravitation under discussion, the only new postu-

late is that some or much of the ether's intrinsic energy is *kinetic* and consists of some sort of wave motion or energy flux, whereby a disturbance at any point in the free ether is ultimately felt everywhere else, diminishing in intensity, of course, with the inverse square of distance from the seat of disturbance.

It is not difficult to conceive of kinetic energy in the ether quite apart from matter. Radiation is one form of such energy, and when once launched in the ether it is persistent and quite independent of its source. Interstellar space is alive with wave energy radiated from countless suns, and at points far removed from any single sun this energy is approximately isotropic. Of course this known isotropic wave energy in the ether of space is far too feeble to play any appreciable part in gravitation, and I call attention to it only for the purpose of showing that one sort of free isotropic wave energy in or of the ether in celestial space is already a known phenomenon.

Probably the ether waves concerned in gravitation are not the transverse kind known to us, though it is not difficult to think of transverse waves of great amplitude, embodying great energy, and of such great length that they pass freely through all bodies without appreciably heating them—even electrical conductors. (Incidentally, we cannot be sure that the intrinsic energy of the ether does not impart some low degree of temperature to matter, because we know of nothing in nature at the absolute zero of temperature or anywhere near it.) It seems more likely, however, that the ether waves of gravitation are longitudinal, or otherwise consist in an energy flux which, by reason of its universal presence, has not been made manifest except by gravitation.

It is easy to understand how the supposed spherical field of influence, or energy shadow, surrounding any body of matter may be initiated, but just how it is maintained may never be known; though I hope to have something to say in this connection in a future discussion. But that the field of influence actually *is* maintained seems certain; gravitation itself is a demonstration of it.

The simplest mental picture of the supposed field of influence which I can think of is a spherical energy shadow, and I have endeavored to make this conception visible in Figs. 2 and 3 as light

Fig. 2

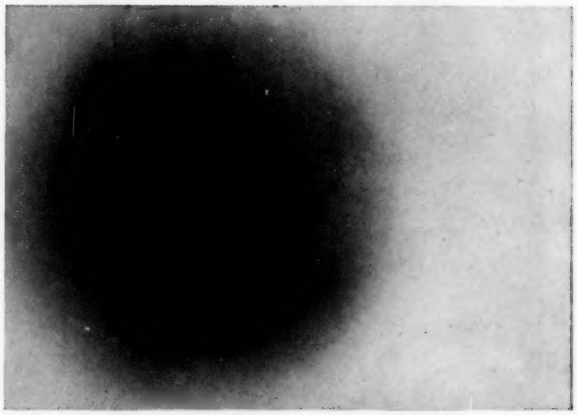
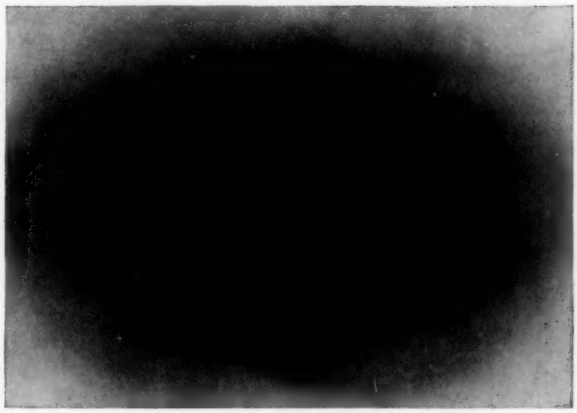


Fig. 3



shadows cast by black spheres. Of course the shadows as here represented are enormously exaggerated. Plate I., Fig. 2, represents a single sphere and the adjacent parts of its spherical shadow. (Obviously a body of *any* shape will cast a shadow substantially spherical.) Plate I., Fig. 3, shows two neighboring spheres with their shadows interblending. The greater depth of shadow between the spheres is clearly indicated, and it is into this deeper shadow that the two gravitating bodies are supposed to be pushed by the superior energy flux from right and left.

Some curious and interesting secondary phenomena are suggested by this conception of the mechanism of gravitation. One of these may be described as follows: Imagine two bodies, such as those of Fig. 1 or Plate I., Fig. 3, falling toward each other by reason of their mutual attraction. They are continually accelerating, and absorbing energy from the ether waves or energy flux pushing them toward each other, whereby these waves pass through and beyond each body *slightly depleted of their energy*, and thus offer less than normal resistance to the advance of the other body; that is to say, the energy shadow between the bodies, into which they are pushed, grows deeper and deeper as they approach, not only because of their lessening distance from each other, but also because of their increasingly rapid transformation of energy as they gain velocity. And it does not matter if the two attracting bodies differ greatly in mass, like the sun and a planet or the earth and a tennis ball, because they will equally acquire momentum, and each will affect the other in the manner described. Stated concisely this means, if my premises are tenable, that Newton's law of inverse squares is not rigidly true for *accelerating* bodies; but that for positively accelerating (approaching) bodies the force of attraction increases a little faster than the inverse square of distance. The force of attraction instead of varying as $1/D^2$ as it does for bodies at rest or in uniform motion, varies as $1/D^{(2-x)}$ for bodies accelerating in the line of attraction, wherein x is a very small quantity which appears to vary with the rate of energy transformation or velocity of fall. When acceleration is negative, that is to say, when energy transformation is *from* the accelerating body *to* the ether, x becomes positive.

Let us consider the effect of x on a planetary orbit: If the orbit is circular, $x=0$ because there is no change of velocity; but if the orbit is excentric, x obviously grows in value and importance with the excentricity, though always equaling zero at aphelion and perihelion. Fig. 4 illustrates the sun and a planet at aphelion in an ex-

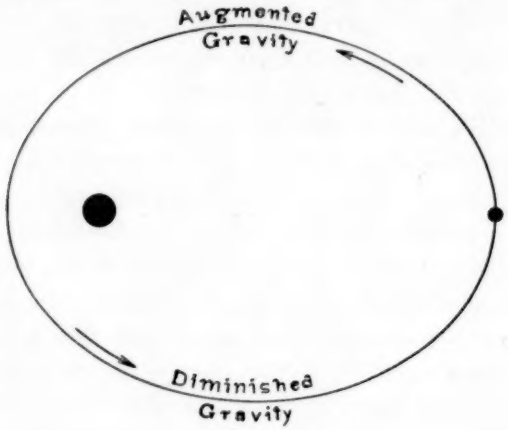


FIG. 4.

aggeratedly excentric orbit. As the planet moves from aphelion to perihelion, normal attraction between the sun and planet is augmented by the positive acceleration of both as before explained; and while the planet moves from perihelion to aphelion normal attraction is diminished by negative acceleration.

If I am not mistaken in my mechanics, the gravitational disturbance above described will slightly change the shape of the orbit, and cause a continual advance in the position of perihelion by advancing the line of apsides. Probably the effect is too small to be detected in the case of any of the planets of the solar system except perhaps Mercury, because of the small excentricity of their orbits; but the high excentricity of Mercury's orbit possibly may reveal it, and I hope it may be found adequate to account for some of the anomalious secular advance of the perihelion of Mercury's orbit. I shall be glad to have my astronomical friends investigate this.

The orbit of the moon is not very excentric, but she moves toward and away from the sun almost the full diameter of her orbit

every month. Perhaps the gravitational disturbance I have suggested may aid in explaining some of her more obscure motions; and I hope it will be found to have a slight accelerating tendency so as to compensate the slight retarding tendency of which I shall treat in the second division of this paper.

2. TRANSMISSION OF GRAVITATION CANNOT BE INSTANTANEOUS.

Laplace at first sought to explain the secular acceleration of the moon's mean motion by ascribing to gravitation a finite velocity of propagation. Later he said:[2] "The time of its transmission, if it were sensible to us, would be particularly evinced in the acceleration of the moon's motion. I suggested this as a means of explaining the acceleration which is observed in this motion; and I have found that in order to satisfy observations we must ascribe to the force of gravity, a velocity seven millions of times greater than that of a ray of light. As the cause of the secular equation of the moon (c) is now well understood, we may affirm that the attraction is transmitted fifty millions of times more rapidly than light. We can therefore assume, without any apprehension of error, that its transmission is instantaneous."

I doubt if anyone who has bestowed careful thought on the subject, in the light of present-day physics, really believes this. To me, it is inconceivable that my change of position, as I walk across a room, is felt among the fixed stars while I am still walking; but the justly great name and fame of Laplace has stamped this dogma with the seal of authority, and for more than a century it has blocked the path of fruitful thought on the physics of gravitation.

Doubtless Laplace made no serious mathematical mistake in reaching his remarkable conclusion, but perhaps he erred in his choice of premises. He postulated[3] a "force" or "gravific fluid," "which rushes towards the sun with an immense rapidity; the resistance which the planet experiences from this current in the direction of the tangent, he conceives to produce a perturbation in the elliptic motion, like to the aberration of light." He then applied this

[2] "System of the World," Harte's translation, Vol. 2, p. 322.

[3] Harte's translation, Vol. 2, notes, p. 490.

conception to the case of the earth and moon. I have endeavored to visualize Laplace's conception in Fig. 5, in which E represents the earth, M the moon moving in the dotted line orbit in the direction indicated by the large arrow, and lines NE the "gravific fluid" rushing from all directions toward the earth. The orbital motion of the moon continually carries her laterally against the stream of "gravific

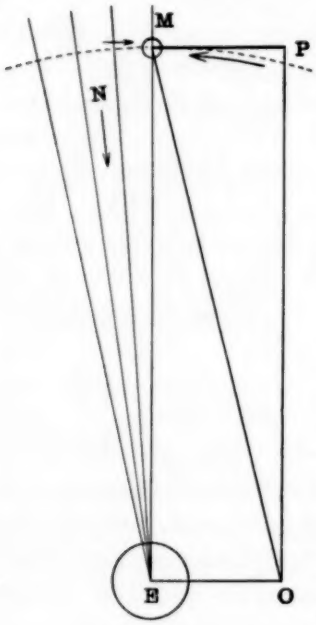


FIG. 5.

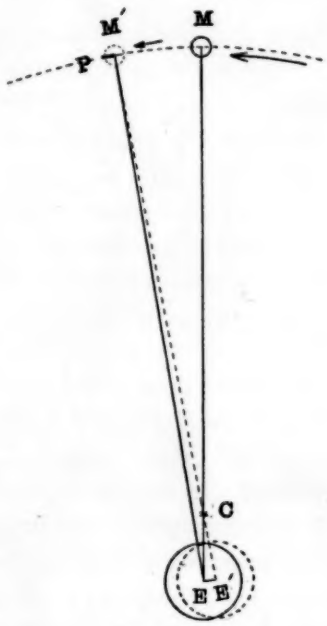


FIG. 6.

fluid," whereby she experiences a tangential retarding force, indicated by the small arrow, just as if a less rapid flow of "gravific fluid" came from that direction. Let the line MP represent the direction and value of the retarding force in terms of the centripetal force ME. Completing the parallelogram of forces, we find the line of the moon's attraction shifted from ME to MO. Clearly this would result in the moon taking an orbit in the form of a contracting spiral which would ultimately bring her to the earth. At the same time her actual velocity would continually increase (and her angular velocity still more so) because of her falling toward the

earth; gravity directly, and the retarding force indirectly conspiring toward this result. With the assumed velocity of light for the "gravific fluid," Laplace found that the angular acceleration of the moon's motion would be millions of times greater than necessary to account for her known acceleration.

I have thus outlined Laplace's conception of the mechanism of gravitation, which led to his famous conclusion of virtually infinite velocity of propagation, because I have not met with anything of the sort in modern text books of astronomy or physics; his startling conclusion is known to everyone, but his premises are generally forgotten; and further because I wish to have it clearly in mind for contrast with what is to follow.

Returning now to the theory of gravitation under discussion: In Fig. 6, E represents the earth and M the moon moving in the dotted line orbit in the direction indicated by the large arrow, both revolving about their common center of gravity C. Instead of showing C well inside the earth's circumference where it belongs, I have shown it outside, so as to enlarge certain details of the diagram and thus avoid confusion of lines.

The earth and moon are each supposed to cast a spherical energy shadow, or occupy the center of a spherical field of influence, as already indicated, into which the other is pushed by the slightly superior energy-flux coming from beyond it. The strongest push of each is toward the densest part or *origin* of the shadow of the other. Thus, if earth and moon were stationary, each would be pushed toward the center of mass of the other. But while the shadow, emanating so to speak, from the earth at any instant is being propagated outward to the moon's orbit, the latter will have moved to M', and the earth will have moved to E'. Clearly then, the moon at M' will not be pushed toward E', but toward E, which is the origin of the shadow into which it is being pushed. The centripetal force $M'E$ may be resolved into the radial component $M'E'$ and the tangential component $M'P$ equal to the displacement of the earth's center from E to E'. Clearly, the force $M'P$ is an *accelerating* force, and bears the same ratio to gravity at M' that $M'P$ bears to $M'E'$.

If the velocity of propagation of the energy shadow equals the

velocity of light, then the displacement of the earth's center from E to E' will be about 52 feet, and the tangential force $M'P$ will be about one twenty-four-millionth of gravity at the distance of the moon.

Obviously, this very small tangential force will tend to make the moon's orbit an expanding spiral of very small pitch; but the vastly greater force of gravity will resist this tendency and nearly, but not quite, counteract it; the net effect being an extremely slow lengthening of the radius vector, and a very slight *retardation* of real as well as angular velocity. This paradoxical effect, of an accelerating force producing an orbital retardation, is explained by Sir George H. Darwin in his chapter on tidal friction and the genesis of the moon.[4]

I have made only a very rough estimate of the secular retardation of the moon's mean motion which this minute accelerating force will bring about, with gravitational transmission taken equal to the velocity of light, but have satisfied myself that it will amount to a very few seconds of arc only, in a century; and I do not claim that the velocity of light is the velocity of gravitational propagation unless the postulated ether waves are ultimately found to be transverse like those of radiation. I think it probable that they are longitudinal, or otherwise different from those of radiation. If this be true, the velocity of propagation may be several times greater than that of light, and the secular retardation of the moon correspondingly less.

I realize that any uncompensated retardation of the moon's motion will add to the present outstanding observed acceleration, if any; but am hopeful that the slight departure from Newton's law of inverse squares already suggested may, in connection with other motions of the moon, supply some of the necessary compensation. There is also a minute source of compensation, due to motion through the ether, which I intend to consider in another discussion.

CLEVELAND,
April, 1914.

[4] "The Tides," Chap. XVI.